# The Journal of the American Medical Association

VOL. XVII. CHICAGO, JULY 18, 1891. No. 3.


## ORIGINAL ARTICLES.

### THE SCIENTIFIC RATIONALE OF MODERN WOUND TREATMENT.

*Read in the Section of Surgery and Anatomy, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May 5-8, 1891.*


BY HENRY O. MARCY, M.D.,

OF BOSTON, MASS.


The currents and counter-currents of surgical opinion, which dominate the methods of wound treatment, are perhaps nowhere better illustrated than at present in the public hospitals of London. In one of these the great founder of antiseptic surgery explains to his pupils and a very considerable number of surgeons, who gather daily in his wards from all parts of the world, the fundamental principles upon which his methods are formulated. These he exemplifies with painstaking care in the great variety of operations which are done in the amphitheatre of a large general hospital. It is noteworthy to observe that drainage is considered far less important than was earlier taught, although every wound is dressed with ample provision for protection from extraneous contamination, with the expectation that there may be, at least, a certain amount of serous or bloody fluid escaping from the wound, which must be prevented from becoming infected. Mr. Lister now feels certain that the double cyanide gauze is by far the best protective dressing that has yet been devised.

In Guy Hospital carbolic spray may be seen in daily use, as a still further protection during the dressing of wounds, which is usually done at the bedside in the wards. In another hospital all this is openly abandoned, and antisepsis is not only considered useless, but harmful. Here, however, it is noteworthy to observe that the so-called system of cleanliness, which is emphasized, is antiseptic so far as it is possible to destroy the septic material with which, in life, all individuals are usually surrounded and must necessarily come in contact. This pertains especially to the field of operation, the operator, his assistants and the material used by them.

In Birmingham the surgeon, most often heard from in unmeasured terms, scouts all the processes pertaining to antiseptic surgery as "illogical and unscientific"; claims that even the vital tissues themselves in a state of health have ever present in them infective material, and that the dominating surgical thought of to-day is already an exploded fallacy, and that the system so elaborately constructed by Professor Lister is "as dead as Julius Cæsar."

In the great hospitals of Berlin there is a singular uniformity of method, revolutionizing all the work of the earlier days, both in technique and results.

In America, where individual opinion has its expression with the greatest degree of freedom, individuality is more noteworthy, and, although the teaching of surgery in the great centers of learning is largely based upon the fundamental principles as elaborated by Sir Joseph Lister, there is too often seen a carelessness of method, especially in detail, and corresponding imperfect result which indicates either a disbelief in or an imperfect knowledge of what, for my own part, I had supposed long since considered demonstrated.

It is wise and ever profitable analytically to review, with critical care, our own convictions and experiences. This is not alone philosophic, but in large measure the way in which advancement may be made, and oftentimes leads to the condemnation and abandonment of our own most cherished ideas, resulting in the adoption of new means by which to reach a given end. Only by such measures can the individual himself become progressive and keep apace with the tide of accumulated observations, with the assimilation of new truths, and himself become an important contributor to the advancement of science.

In this spirit, and not as a partisan, do I purpose to claim the attention of the section to a brief discussion of the scientific rationâle of wound treatment, necessarily brief, since it were easier to write a volume than to attempt a correct presentation of the subject in the short time at my disposal.

So omnipresent is the appearance of vital activity in all organic material that we cannot wonder that it long went unchallenged as an inherent factor. The ever illusive search for the beginnings of life led to the belief in a spontaneous generation, under favorable conditions, and to the

solution of this problem we are first of all indebted, as the foundation of subsequent demonstration. Here Pasteur, Tyndall and many others of scarcely less note laid the foundations of a practical science as wide-reaching as humanity, the importance of which hardly dawned upon their perception. In this connection it affords me the greatest pleasure to pay tribute to one of my earliest and most loved teachers, the late Prof. Jeffries Wyman, of Cambridge. In the *American Journal of Science and Arts*, vol. 34, July, 1862, Prof. Wyman gave an account of some experiments on the formation of infusoria in boiled solutions of organic matter, the result of which was that such solutions, exposed only to air which had passed through iron tubes, heated to redness, became the seat of infusorial life; the same results followed when similar solutions were enclosed in hermetically sealed flasks, and subsequently exposed to the action, for a short period, of boiling water. In a few instances, infusoria appeared when the temperature was raised above 212° F.

He says: "All living beings found under the above circumstances have been attributed either, 1st, to organisms, or the germs of them, supposed to be contained in the fluid experimented with, or the air included in the flasks; or 2nd, to the direct transformation of organic matter into new living beings, independently of any germs, or living organisms whatever; or in other words, to 'spontaneous generation.' Abundant proof has been brought forward to show that the spores or germs of infusoria exist in the air in quantities amply sufficient to account for the presence of living organisms in solutions freely exposed." . . . "There can therefore be no certainty of the existence of spontaneous generation in a given solution, until it can be shown, that this has been freed of all living organisms which it contained at the beginning of the experiment, and kept free of all such from without during the progress of it. On the other hand, this kind of generation becomes probable, whenever it is made certain that infusoria do appear in solutions, in which the conditions just mentioned have been complied with."

For the purpose of determining whether infusoria can develop in organic fluids freed from living organisms which are kept secluded from atmospheric contact, Dr. Wyman entered into a long series of experiments with boiled solutions of organic matter in sealed flasks, which experiments were published in the *American Journal of Science and Arts*, September, 1867. The material used was the albuminoid product, usually boiled and filtered beef juice. The experiments were twenty in number and each usually consisted of a series of flasks, subject to a great variety of exposures. The contents of the flasks were afterward carefully studied by himself and Prof. Henry J. Clarke, and the different varieties of bacteria were diagrammed, as seen under the microscope, enlarged from two to four thousand diameters. Prof. Wyman arrived at the conclusion, that at the temperature of boiling water, when continued for a sufficient period, the organic solutions contained in the flasks were completely sterilized, and that no matter what the surroundings or conditions, the fluid remained sterile until again brought into contact with the atmosphere. Under recent date, Dr. Morrill Wyman, of Cambridge, brother of the late Professor Jeffries Wyman, wrote me that some of the flasks used in these original experiments still remain with contents free from any evidence of life.

These original researches by one of America's most distinguished investigators are worthy of permanent record in the relation which they bear to antiseptic surgery. It seemed but a logical sequence that the introduction of these low forms of organic life into the albuminoid secretions of wounds would there germinate and be the legitimate cause of subsequent decomposition, producing all the train of evils incident to suppurating wounds.

Having once demonstrated that the cause of decomposition in organic fluids was due to an extraneous something, usually everywhere present, and that *that something* consisted of certain definite forms of life introduced into them from without, these factors were plainly applicable to the treatment of wounds, and in this commenced the monumental labors of Mr. Lister.

The question, naturally, soon broadened out to determine, if possible, the varieties of bacteria which develop within the organism during the life of the higher animals, the peculiar conditions incident to their growth, and if certain varieties were more harmful than others. In other words, it became necessary to study *de novo* certain chapters of natural history, involving several families of the lowest orders of plant life. All this is now recognized as so important that bacteriological researches, from the standpoint of laboratory investigations, are included in the curriculæ of all the better equipped universities. These additions having been made to science, it naturally followed that, when the conditions of bacterial reproduction were known, it was then, and not *until then*, that the investigations of changed relationships which would limit, restrict, or prevent, their development could be intelligently undertoken.

Many of these investigations have borne fruitage of the greatest value to the human race. We now have the scientific demonstration of this in man, in the easy control of small pox by vaccinia, established by the immortal Jenner, in the lower animals, in the valuable illustration of the same teaching by Pasteur in chicken cholera, and in the more recent and by far more important

original investigations and demonstrations by Dr. Frank Billings, of Nebraska, in the limitation and control of hog cholera.

Very naturally this line of research has been extended, until it may be accepted as demonstrated that the large share of contagious and infectious diseases to which the human race is liable, are due to the introduction within the organism of a variety of spore plant growth, the development of which is the causative agent of the given disease. I need only to refer to the oft-repeated and generally accepted demonstrations of these important truths as illustrated in anthrax, cholera, tuberculosis, pneumonia, diphtheria, typhoid fever, measles, scarlet fever, etc.

Although quite a variety of other forms of growth may be introduced into the human system and germinate to the production of disease and death through the medium of an open wound, as for instance, anthrax, or diphtheria, it was speedily demonstrated that the poisoning of wounds was chiefly due to the micrococcal or round-celled growth. It was shown that these could be isolated, cultivated artificially, that they bred true, and upon reintroduction into wounds they always produced the same general class of symptoms and poisoning. Familiar illustration of this is found in erysipelas.

To show the fallacy that all organized material possesses in itself the means by which decomposition may go on spontaneously, a, so to-speak, inherent bacterial infection of low order, the early experimental observations of Prof. Wyman already alluded to, then demonstrated that the vital organisms contained in organic fluids were destroyed by the simple process of prolonged boiling, or by a retention for a considerable period at the heat point not above 212° F. A much more simple series of experiments unquestionably show that organic fluids, introduced into sterilized flasks without bacterial contamination, do not undergo decomposition, although freely exposed to atmospheric contact, provided that there is a protection of a slight packing of sterilized cotton which serves to filter out the germs of decomposition commonly present in the atmosphere. This is easily demonstrated in urine or blood exposed to very considerable elevations of temperature, and these highly organized fluids remain sterile, unchanged indefinitely. The exposure of these same fluids to the atmosphere of a common living room, by the simple removal, for a few minutes, of the cotton protective, is sufficient to cause a rapid decomposition to ensue by the development of atmospheric germs coming in contact with them. This is shown to be a fundamental fact, pertaining not only to the fluids of the body, but equally also to the organized tissues, when treated in a manner that shall exclude from the same the extraneous germs of decomposition.

In reply to an able and exhaustive argument by Professor James L. White, of Philadelphia, upon the present position of antiseptic surgery, Mr. Tait, of Birmingham, publishes in the *British Medical Journal*, February 14, 1891, a defense of his position, from which I am constrained to make an extract, since Mr. Tait is so often quoted as an authority to prove the uselessness of what is called the antiseptic system of wound treatment: "Fortunately for my present purpose, Prof. White puts the issue syllogistically, and formulates for both of us a major premise upon the truth or error of which depends the whole conclusion; and I accept this issue freely. I say that germs of decomposition exist already in the blood and elsewhere in the body and are ever present, but do not bring about their results till death, or some condition which we may call a tendency to death, gives them permission so to do. Professor White says that the elaborate and carefully conducted experiments of Houser, Watson, Cheyne, and others completely contradict the statement 'which is really the foundation of Mr. Tait's argument.' In reply, I say I care not a straw for elaborate and carefully conducted experiments, no matter at whose hands, when their conclusions are diametrically opposed to every day experience." . . . "In truth the facts of the housekeeper and the henwife are far more scientific, that is, far more exact than those of our biological experimenters. They are in harmony with what I see in my work every day and therefore it seems to me a perfect waste of time to follow Professor White beyond his own major premise, which is utterly mistaken."

It may be accepted that the proof of the mistake in the major premise, as based upon such demonstrations, will not be received by the profession, as scientific, or determined, and to me it seems, in large measure, a waste of time to attempt to refute them. Really the housekeeper bases the entire art of preservation of all her organic compounds upon the demonstrations of Professor Wyman, already referred to. Upon these has developed the preservation of the food products, by the canning system in use in the various parts of the world. In America, the meats of Texas, the fruits of California, the salmon of Alaska, are familiar illustrations of great economic commercial value in supplying our daily wants, giving employment to thousands of people and furnishing a product of an annual value of millions of dollars.

We might broaden the inquiry to the natural decomposition of fruits and vegetables, including all the grains. Break the epidermal cells of the skin of the grape, the orange, pear, or apple, and all know the rotting of the fruits by the rapid development of a low order of new growths. All the starchy seeds, bulbs, and grains, are protected by an impermeable envelope during their so-called sound state. There is no vital force which

holds in preservation these starch cells, they are simply stored up, like the albuminoid products of the egg, to serve as food in the development of the germinal cells in the earlier stages of reproduction, until the plant has reached a stage of evolution with sufficient inherent power for an independent existence.

If I understand at all what is meant by the demonstration of the henwife, it is simply this and nothing more, that the vitalized ovum is surrounded by a mass of nutritive material, enclosed for its better protection in a porous casement of lime salts, which albuminoid material contained therein is utilized for the development of the growing chick and dominated by vital forces, which elements become parts of a vital organization in precisely the same manner as when the independent individual utilizes its food, which in later life is received into the organism through the alimentary canal. The chief difference that pertains in the characteristics of the growing organism, is that in the oviparous animals the nutriment necessary for the development of the ovum is emitted in a mass sufficient for the purposes of the economy of complete development, while in the mammalian vertebrates the fœtal nutrition, by the process of secretion and absorption, is elaborated pari-passu to meet the necessities of the growing organism. In the one instance, the egg, when devitalized, speedily undergoes bacterial decomposition, introduced through the protecting envelope from without. In the other, as Mr. Tait should well know, the fœtus, having perished within the uterus, undergoes a process of maceration, but never decomposition, unless by the introduction of germs from without. Therefore, it is evident that Mr. Tait's major premise, as he understands it, based upon the scientific deductions of the housekeeper and henwife, are subject to revision, and indeed, when carefully studied, are strictly in accord with the scientific observations, as daily conducted in our better laboratories.

The problem confronting the surgeon is easily subdivided into the two chief factors viewed from the aspect of strict science. The first factor, based upon the accepted premise which I am sure few would have the hardihood to consider "illogical and unscientific," that the vital organism with which surgery has to deal is, itself, in health, free from bacterial ferment, protected irom without by a coat of mail, when unbroken impermeable to invasion, and from within through its mucous surfaces by a similar disposition of protective cells, is that all operative wounds made in such tissues should be, as far as possible, made and maintained aseptic; the second, when septic, to determine the best measures for the destruction of the infecting organisms. The wounds of the first class are liable to extraneous infection from everything with which they may come in contact, and hence the necessity for sterilizing the surroundings and the material applied. It is sufficient for our present purpose to omit reference to the manipulative detail, as to the means to be used to secure this end. I cannot myself doubt, but that the justly discarded and much abused carbolic spray, in the earlier stages of the problem, served as a valuable means in securing good results. Irrigation serves a far better purpose and in the present state of our knowledge cannot safely be dispensed with. A wound in uninfected tissues should be aseptic, as far as possible freed from devitalized structures, carefully approximated, and held at rest. It should be maintained aseptic by the application of suitable dressings.

The second factor of the problem is a much more difficult one. How to treat a wound made in tissues which are already infected, or to best care for a wound which has by accident become the seat of septic ferment, is a subject upon which there is great difference of opinion. Anything like a satisfactory discussion of the problem confronting us demands the careful study of the varying conditions of the individual, the vital, resistant power of the infected organism, as well as the character and amount of the infection itself. The individualistic resistant force is a constantly fluctuating factor, naturally greater in young life and lessening in accordance with age, surroundings, habits, etc. Bacterial growth in dead tissue, under favorable conditions, goes on to the entire decomposition of the material.

When introduced into the living tissues in small quantity, although the heat point and albuminoid products for its food are ever present, the vital, resistant power of the organism may be such that the bacterial ferment will not germinate at all, or in such a limited degree that at the most the disturbing conditions are strictly local and soon disappear. Under favorable circumstances, the resultant suffering and danger are in direct ratio to the amount of the infected material introduced into the organism, and this may be in such quantity as to entirely overcome the resistant power and rapidly cause its destruction. The character of the seed introduced also varies greatly, the reproducing power of some ferments, as anthrax, being such as no matter how small the quantity greatly to endanger the individual. Not seldom the prick of a needle, carrying a minute portion of bacterial infection from a dissection wound, may introduce a virus into certain localities, where its rapid reproduction produces speedy death, while many of the atmospheric germs, common to all localities, reproduce only feebly under the most favorable conditions and soon disappear.

In tracing out the processes which a healthy organism has at its command, it is of intense interest to note the manner in which Nature rallies

her forces for resisting invasion. The profession have long been familiar with the rapid proliferation of cell character which goes on about a wound, and these changes have been more or less carefully studied from the beginnings of surgery. My earliest teacher in medicine, the late George A. Otis, Surgeon of the United States Army, whose monumental labors in the elaboration of the surgical history of the late War of the Rebellion won for him enduring fame, emphasized the observation that the so-called pyogenic membrane in abscesses was in reality protective to the surrounding parts and was not to be interfered with by surgical manipulation.

We now know that the leucocytes, so familiar to all versed in histological study, are endowed with a peculiar physiological power. One of the first processes which we are wont to observe under the name of inflammation, we find consists in a rapid proliferation of the white cells about the point of invasion, encapsuling, as it were, the foreign material. That they do more than this, having the power to surround and destroy, so to speak, under favorable conditions to digest the bacteria is now generally well known, thanks to the observations of Metschnikoff. Although it is quite too early to draw general deductions from these facts and declare that the entire solution of the so-called vital, resisting power of the tissues lies in this power of the leucocytes, there is every reason to believe that this important discovery gives an explanation of satisfactory type to certain of the hitherto unexplained factors in the repair processes of wounds. In the repair of the minor subcutaneous injuries we have familiar illustrations of the part which the leucocytes play in the animal economy. The effused blood is surrounded by them, and the material of the exudate is appropriated for their own development. Minute capillary vessels are formed in the line of these invading cells and the process of clot disappearance and granulation tissue development go on pari-passu until the clot has disappeared and new connective tissue restores the part to its former condition.

When a small colony of micrococcal cells have found lodgment, the leucocytes surround and shut in the enemy, until a wall of living granulation cells are formed, forcing it to surrender, and a localised abscess is the sum total of damage.

If we find in these familiar leucocytes the so-called phagocytes of Metschnikoff, empowered to a certain extent with the ability actually to destroy infecting bacteria, we certainly have in a very considerable measure an explanation of the vital resisting power of the individual organism. If, under favorable circumstances, these canabalistic little workmen, not alone surround, but actually eat up, their enemies, we have the best of reasons for understanding why the comparatively few germs in the atmosphere of an healthy locality are far less dangerous to wounds than was earlier supposed.

Again we understand why in the so-called sugically clean wound, a wound where great care is taken to exclude foreign material, and the comparatively uninjured clean-cut surfaces are closely approximated, the reparative processes go on steadily, and rapid recovery supervenes, although in a strictly scientific sense, the wound may not be aseptic. The infected organism suffers in a two-fold manner. First, locally, that is, in the wounded surface and its immediate neighborhood, and in a general constitutional poisoning. The latter is produced by a chemical substance capable of isolation, called sepsin. This may in itself be sufficient to cause death, and its importance can hardly be overestimated, but since it is the direct result of microörganic development, it follows that the control, or destruction, of the organic ferments is the sure way of cutting off the septic, systemic suffering.

In this process, the methods which have been emphasized as aseptic, or surgical cleanliness as in contra-distinction to antisepsis, bear no part except so far as they may aid in removing the products of decomposition. The prerequisite knowledge in this instance must be sufficient to enable the best trained surgeon to make use of those agents best fitted to destroy the organisms in loco with the least possible injury. Hence the wide field of research of a most painstaking and scientific character which must of necessity be traversed before a satisfactory solution of this question could be given. Now, we have the fruitage of a multitude of patient investigators; notably Mr. Lister and Mr. Cheyne, of England, Pasteur in France, Koch in Germany, Sternberg, Cabot and others in America. My own publications upon this subject were the fruitage of two years of carefully conducted laboratory experiments.

The mercuric-bichloride solutions are usually greatly to be preferred as more effective as a destroyer of infection and less irritant to the wounded surface. All pockets in suppurating wounds must be carefully cleansed and drained, while in iodoform we fortunately have an agent, the crystals of which are only in a minor degree irritant, and as such absolutely non-poisonous. They dissolve very slowly and are germicidal only in solution. Hence by their use bacterial development is greatly retarded, if not altogether prevented. The iodoform crystals in most wound secretions, as ordinarily observed, dissolve so slowly that the antiseptic power is continued as a constant factor for hours, sometimes even for days together.

From this hasty, and necessarily imperfect, review of a subject second to none in interest or importance to our entire profession, we have

omitted of necessity very much of value. However, I am certain that "the better methods of wound treatment have a fundamental basis of a strictly scientific character, the three important factors of which are: *First*, the vital resistant power of the individual. *Second*, the character and amount of the bacterial infection. *Third*, the local condition of the tissues at the seat of implantation.

The ideal treatment of wounds is certainly the restoration of the condition of the parts operated upon to, as nearly as possible, their primal state. If this can be effected aseptically, then there are no bacteria to be removed, and if the wound is surgically clean, with accurate coaptation of the sundered parts, then the vital forces are sufficient to utilize any resultant exudates, and drainage is not alone superfluous, but harmful. The reparative process should go on under a dressing which will permit of the introduction of no foreign factorage. The various antiseptic dressings now so generally used, have a value in wounds necessarily drained, that is subject to a probable infection, but in aseptic wounds primarily closed, they are unnecessary, expensive and cumbersome."

For quite twenty years I have been in the constant practice of closing *aseptic* wounds by lines of buried animal sutures. Little by little I have been led to discard drainage, almost without exception, in this entire class of wounds, until I have formulated it as a rule of practice, that no matter how large or deep in non-infected structures, the sundered parts are rejoined, oftentimes by the use of several feet of tendon suture, and the wound is sealed by a germ-proof dressing of collodion. To this rule the larger amputations are no exception, and such wounds thus treated rapidly heal without pain or œdema of the coapted tissues. In reality, it makes little difference by what name we call this modern miracle of surgical wonder-working. It has its establishment upon the foundations of pure science, its future gives promise of still greater achievement, and we may look forward with confident expectancy to the day when medicine and science, although they cannot be axact sciences, will be understood and practiced with a scientific rationâle above contradiction or reproach.

My brethren of a noble profession, second not even to that of the clergy, may I not be pardoned a digression? We have spent the last two days in the Councils of the Academy of Medicine intent upon the purpose of elevating the profession to a higher standard of education, comparing the advantages of a general, with those of a technical training. May we not accept that they both should be broadened in the Catholicity of science which is after all the demonstration of God's own law of pure and simple truth? The one profession reads it in the Divine revelation transmitted through the Fathers; the other is the unerring law of vital forces transmitted from the beginning of creation. The one teaches a mediatorial redemption from the transgressions of the moral law; the other knows of no high-road cast up for the escape from the penalties attached to the infraction of its inflexible government.

Since both emanate from a common source and center of Being, should they not be at least equally respected, investigated and obeyed? He who studies the infinitely minute, and yet sees through it all the marvelous working of a vital law, intended primarily for the beneficient good of the created, is moved by the same divine inspiration as he who measures the infinities of space and weighs the distant stars in balances of mathematical correctness. If I read aright the doctrines of our great republic in the evolution of our race, the time is not far distant when the necessity of sending our young men to the great centres of European thought for a higher development in the training and knowledge of the exact sciences will have passed.

Let our own central government not alone foster the education of the masses as a bulwark of defence in the protection of our own inherent safety, but let her also provide national laboratories which shall amplify the advantages of the Carnegie and John Hopkins institutions for biological researches, the advantages of which to our entire race can hardly be overestimated.

---

## EMBOLISM OF A BRANCH OF THE PULMONARY ARTERY. PATIENT SUSTAINED BY OXYGEN GAS. RECOVERY.

*Read in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D.C., May, 1891.*

BY GEO. E. FELL, M.D., F.R.M., Sy.,

LATE PRESIDENT AMERICAN SOCIETY MICROSCOPISTS, PROF. PHYSIOLOGY AND MICROSCOPY MED. DEPT., NIAGARA UNIVERSITY, BUFFALO, N. Y.

One of the remarkable conditions associated with the blood vascular system is, that with all the radiations of the capillary system, we have so little disturbance in the blocking of these minute channels. It is true that without some external influence acting on the coats of the vessel, or some modification of the blood, or walls of the vessels through and by which the fibrin factors are disturbed in their normal relations, we could not readily ascribe a cause for the accumulation of any substance which would block or disturb the activity of the circulation. We do, however, have modifications of the blood, as in albuminuria, which appears to permit such abnormal changes to exist, so that the fibrin-forming factors take on an abnormal state, with the conse-

quence of the formation of a greater or less amount of fibrin, resulting in a clot. The size of this clot, of course, has a very great bearing upon the prognosis in these conditions. In the pregnant state, with albuminuria preceding labor, we have just such a condition existing whereby the excessive strain upon the uterine vessel, and the more or less mechanical effect produced by the passage of the fœtus, we may have the vessel so disturbed that a clot may readily form. The history of the subject gives us the authority to state, that emboli do form, and frequently to a very great extent, so that the functions of important vessels in various parts of the organism are interfered with, thus disturbing to a greater or less degree the physiological relations of the parts affected. It may be stated in passing, that clots of so large a size have been formed that on entering the right side of the heart they have immediately disturbed the function of this organ so as to produce immediate death.

When the embolus passes into the pulmonary artery so as to completely occlude this important vessel, of course death must immediately ensue. However, we may have many conditions existing wherein only portions of the vascular area, to which this important vessel is distributed, are disturbed, and I now have the pleasure of being able to report a case in which all the evidences point to such a condition. Before, however, entering upon the details of this case, we will refer briefly to the results of emboli forming in various parts of the body. The result of the occlusion of a small artery is simply told in the following words, modified from Coles' Histology.

"In embolism a small mass of coagulated blood or fibrin, a small mass of tissue separated from an organ by ulceration (endarteritic modifications), some extraneous matter penetrating into the vessels in some way or other and carried away by the blood stream, on reaching the small branches of the arterial system, become impacked and suddenly interrupt circulation for a time in the area supplied by the vessel plugged. In the same manner thrombosis and the addition of coagulum products would cause more or less complete anæmia of the area supplied by the obliterated vessels. The effects of the occlusion of arteries which do not freely anastamose with other arteries of the same size, so-called terminal arteries, are simple to understand if we keep in mind the way in which blood circulates through an organ thus supplied. The blood passes from an artery into smaller arteries, and finally into arterioles. These arterioles end in a plexus of capillaries (or in the lungs in the wonderfully minute ramifications in the air vesicles). From these capillaries veins corresponding with the arteries originate, but although a terminal artery does not anastamose with other arteries, the capillary plexus belonging to it is continuous with the capillary plexus corresponding to neighboring arteries. "When one of these arteries is obstructed from some cause or other, the first effect is the stoppage of the flow of blood through all its branches, and in the lungs, consequently, a less area of respiratory surface is produced." The blood remains nearly stagnant in all the area supplied, except at the periphery where the capillaries join to adjacent arterial systems. A very slight amount of blood penetrating through these collateral vessels into the stagnant area; blood still passes very slowly from it into the corresponding vein, but the circulation is so sluggish that the walls of the vessels lose their vitality by degrees, and consequently their tone. They become more or less distended, and in this stage the infarct may assume the appearance of a hæmorrhagic block. However, stagnation is soon followed by coagulation and complete arrest of blood flow, and in organs like the kidneys, liver, brain or spleen, the tissues and the blood itself degenerate as the functional and organic circulation are associated in the same vessels. "In the lungs, however, owing to the bronchial vessels supplying nutrition, there is less tendency to necrotic changes. In the former organs the blood becomes granular and discolorized." At the periphery of the infarct other phenomena take place. They are due partly to the increased blood pressure in the collateral vessels resulting from the increased supply caused by the obliteration of one of the channels through which the blood had to pass, and partly (and probably chiefly), to the modified and sluggish state of the circulation in the capillary zone common to the system of the obliterated artery and that of the neighboring arteries. In that zone of sluggish circulation leucocytes accumulate in the vessels, diapedesis takes place and causes infiltration of the tissue. Small hæmorrhages are very common, and the tissues are in a low inflammatory state. It is evident that part of this zone belongs to the area supplied by the occluded artery; and in sections of hæmorrhagic infarcts it is easy to demonstrate that this zone is supplied by the collateral capillary circulation."

This description of the action of an embolus refers to the kidney structure, and it will apply equally well to similar conditions existing in branches of the pulmonary artery. In the latter case we have the circulation cut off to a greater or less extent from the air vesicles supplied by the occluded artery and its branches, thus disturbing, according to the extent of area affected, respiration to a greater or less degree. If the blood cannot pass through the air vesicles of a sufficiently large area, dyspnœa is at once produced. In addition to this, owing to a lessened area of respiratory surface, cyanosis may ensue also to a greater or less extent. It is of course easy to imagine conditions in which the area of

disturbed respiratory surface is so small that the respiratory function is not interfered with to such a degree as to be inimical to life. The grade of disturbance, as stated, may vary between that condition which produces sudden death, and that which produces but a temporary disturbance to the system. In the latter case we may imagine the infarct to have been small in extent, and the changes taking place in it being of such a character that it is absorbed, thus doing away with any serious results supervening through its presence. However, the case which I will relate to you to-day, was of a very serious nature, and only through the happy thought of supplying the patient with oxygen was the decreased respiratory surface enabled to supply sufficient oxygen to the blood to retain the vitality of the system. The value of the treatment cannot be gainsaid. Had oxygen not been used, the result would have been fatal within a very short time, and another case would have been added as the result of embolism following child-birth. How many cases which have occurred in the past might have been saved by the prompt administration of oxygen, of course cannot be surmised, but its evident value was so clearly marked in this instance that it will always be one of the important agents to be recommended in similar cases. Where cyanosis exists oxygen should always be supplied either through the administration of the pure atmosphere, as in cases of paralysis of the respiratory centers, or in cases where the amount of oxygen passed to the lungs is diminished in quantity. An instance of the latter was furnished in a case of membranous croup which occurred with a patient of Dr. Dorr, on Fillmore avenue, this city, in which I suggested oxygen. In this instance, while we did not have a decreased area of respiratory surface, we had a diminished amount of oxygen supplied through the plug of mucus, or membrane, in the trachea. The oxygen administered made up for the loss of air passing into the lungs, and I believe Dr. Dorr will bear me out in stating that his patient's life was saved by the excess of oxygen administered over that ordinarily present in the atmosphere.

In the case of embolism, we had a decreased respiratory surface, and through the supply of oxygen we were enabled to permit this decreased surface to supply sufficient oxygen to keep up the life of the system.[1]

Friday, March 3, 1891, about 2 P.M., while engaged in my office work, I was called hurriedly to attend Mrs. J., a sister of an intimate friend, and was informed that Dr. R. L. Banta, the attending physician, desired me to consult with him. I was informed by the brother and husband of the lady that it was a serious case, and accordingly turned my patients out of my office, and hurried to the residence of the patient. On arriving at the house I found the patient in a hazrdous condition; dyspnœa very great; cyanosis marked. Dr. Banta, however, was not present. I asked for him, and was informed that he would return in an hour or two. Seeing that the case was one calling for prompt action, and making a diagnosis at once, I sent my carriage with a prescription for oxygen gas, and made no attempt to relieve the patient; as little seemed to offer that could be of service. Within a short time a 40-gallon cylinder of oxygen arrived, and I immediately administered oxygen to the patient. The result was most satisfactory. The relief was immediate so far as the cyanosis was concerned. Dyspnœa, however, continued; rapid heart action and respiration, indicating a diminished area of lung tissue in condition for respiration. From subsequent conversation with the patient, the location and the cause of the trouble were defined. She said: "I could put my finger right on the spot where the trouble existed." Following this the area of the disturbance increased so that a greater portion of lung tissue was involved, and the patient was unable for one or two days after to specify the particular point of disturbance, that is, so far as consciousness would permit. More oxygen was procured. A 100-gallon cylinder was obtained, and about 5 P.M., some three hours after my arrival, it was found necessary to use this, the first cylinder having been depleted. The very short time which elapsed in changing the inhaling apparatus from one cylinder to the other, was sufficient to produce general cyanosis, and I hazard the opinion that at this time the patient would not have lived 30 minutes without oxygen. If in one minute deep cyanosis was produced, I think we have reason to believe that death would have ensued in thirty.

While changing this cylinder Dr. Banta appeared upon the scene, and I found by this time that he had not called me in consultation. He showed considerable surprise at my presence, and deemed himself unjustly treated, not having been notified in fact that a consultation was called. Upon explanation, however, that only on being informed that as physician in charge he had called me in consultation, was I present. To obtain my presence I had been deliberately misinformed. He very graciously accepted the situation existing, approved of the use of oxygen, and together we endeavored to make the best of it, both remaining at the house of the patient through the entire afternoon and night. At no time Friday night or Saturday morning could the patient be left without the oxygen without great discomfort immediately ensuing. Through Saturday the condition remained nearly the same, although with a gradual improvement. Every hour's

[1] Five or six physicians who saw the case in the first 48 hours gave a fatal prognosis, with which the author agreed until the wonderful effects of the oxygen gas and subsequent treatment changed his views.

duration of the case gave us reason to hope that what had been prognosticated by five or six physicians in the first few days as a fatal case, might yet possibly result in recovery. Through Saturday night the case progressed favorably, the gradual improvement continuing so that the patient was enabled to breathe for some little time together without the aid of oxygen. Sunday the area of disturbance of lung tissue was increased to its greatest extent, and Sunday afternoon and night the improvement was evident, although the rapidity of the pulse and respirations kept up, so that it appeared the system would be unable to resist the great strain to which it was being subjected. Sunday night, however, the improvement continued, and Monday morning the patient presented a condition calling for an uncertain prognosis, but yet, as life had existed so long, there was reason to hope that changes might ensue in the infarct which would permit the lungs to resume their normal relation. The area of disturbance included, in its greatest extent, probably about one-third to two-thirds of the whole area of the respiratory surface of the right lung. This was indicated by a slightly œdematous condition of the lung tissue. At no time was the disturbance marked in the left lung, the respiratory murmur being distinct throughout its entire area.

Some strong points in evidence of embolism in this case, were the defined circumscribed disturbance at the beginning of the trouble, the increased area of disturbance gradually following the occlusion of the branch of the artery; the rapidity of the heart action, and of respiratory action, —the former ranging from 170, the first day of the condition, to 150, 140 and 130, upon the third and fourth days,—the respiratory action remaining during the occlusion of the area of respiration of the right lung, at about 50 for several days, decreasing to 40, and then gradually coming down to 30, 24, and to nearly the normal respiratory action about one week following the onset of the trouble.

The patient during the first week of illness was unable to lie upon her left side, or upon the back, with any degree of comfort. The first four days the slightest attempt to lie upon the back, or the left side, was followed by immediate dyspnœa—all these conditions indicating the correctness of the diagnosis, and precluding any favorable argument in support of a purely nervous disturbance, as has been suggested by one of the physicians only who was present during this interesting case.

The lacteal secretion, the locheal discharge, and many of the conditions, or features, following ordinary childbirth, were absent during the five days following the confinement. Albuminuria existed to a greater or less degree for a fortnight following confinement. The absence of the locheal discharge was considered of so serious a nature that steps were taken to irrigate the uterus. These measures resulted in establishing the flow, and benefited the patient to a great extent. As stated, the patient was unable to lie upon any other than the right side, or upon the back. The irrigation of the uterus was therefore an interesting problem, and the method which I have long used and first reported to the Buffalo Obstetrical Club and which was published in the Transactions of the New York State Medical Association, Vol. 6, 1889, the latero-dorsal (not Sim's position) came into play in a remarkably interesting manner.

The patient was placed on the left side of the bed, lying upon her right side in the Sim's position. The speculum was introduced, and the irrigating tube passed into the womb. The knees were then raised as described in my article, thus permitting the outflow to pass into receptacle provided for it, and the womb was thoroughly irrigated, with the result that immediately the patient was improved, and in two days the locheal discharge was presenting in its characteristic manner. At this time, as stated, no milk was present. On using the breast pump, applied with Allen's surgical pump, and considerable vacuum, a serous discharge, very small in amount, would pass from the nipples. This treatment was kept up day after day, and followed by mild faradization; and within a few days the glands were secreting milk; and within a fortnight from the time of confinement, the mother had the satisfaction of being able to nurse her offspring.

I am glad to be enabled to report a case of this kind, and one which may have a far-reaching influence, resulting in the saving, possibly, of many mothers whose lives might be sacrificed were it not known that in oxygen gas we have an agent which will tide over often, as it did in this case, conditions which would prove fatal without it.

On the Monday following confinement, three days after I first saw this case, a Dr. E. was called, and suggested the addition of nitrous oxide, with what may be looked upon as favorable results. However, I do not consider it probable that the patient, who had been kept alive more than three days with the oxygen, and had used about 1200 gallons of the gas, would have made any the less a good recovery had the nitrogen not been introduced. This physician also stated that the patient was being burnt up by oxygen, but as her temperature at no time was above 99° F. there could be no grounds for such a statement, and the lady's recovery can alone be ascribed to the use of oxygen gas and the treatment instituted to overcome the albuminuria.

Among working men venereal disease is rare in Paris. Thus a late examination of about twenty-five hundred railroad employés showed but three cases of such disease.

## TREATMENT OF POSTERIOR FACE PRESENTATION.

*Read in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, at Washington, D. C., May, 1891.*

BY EUGENE P. BERNARDY, M.D.,
OF PHILADELPHIA, PA.

Face presentations are rare, and as a result their mechanism and treatment are not always set out clearly by the different writers.

Dr. A. Charpentier gives the following statistics of face presentations:

| | | | |
|---|---|---|---|
| Mme. Lachapelle, | in 15,779 labors, | 72 | times. |
| P. Dabois . . . . | " 2,022 " | 11 | " |
| Braun . . . . . | " 7,835 " | 44 | " |
| At Wurtsburg . . | " 8,514 " | 58 | " |
| At Gottengen . . | " 7,104 " | 29 | " |
| Depaul . . . . . | " 16,233 " | 33 | " |
| Pinard . . . . . | " 81,711 " | 330 | " |
| Total . . . | 139,198 | 577 | |

Average, 1 in 241.

Churchill's statistics for face presentation for British, French and German practice are as follows:

British practice:

| | | | |
|---|---|---|---|
| Dr. Jos. Clark . . | in 10,387 labors, | 44 | times. |
| Dr. Merriman . . | " 2,947 " | 10 | " |
| Dr. Granville . . | " 640 " | 1 | " |
| Dr. S. Cussack . . | " 701 " | 3 | " |
| Dr. Mansell . . . | " 839 " | 7 | " |
| Dr. Thos. Beatty . | " 1,184 " | 4 | " |
| Dr. Collins. . . . | " 16,414 " | 3 | " |
| Mr. Gregory . . | " 691 " | 2 | " |
| Dr. Reid . . . . | " 3,250 " | 15 | " |
| Mr. Lever . . . . | " 4,666 " | 24 | " |
| Total . . . . | 41,719 | 143 | |

Average, 1 in 291⅔.

French practice:

| | | | |
|---|---|---|---|
| Mad. Boivin . . . | in 20,517 labors, | 74 | times. |
| Mad. Lachapelle. | " 15,652 " | 65 | " |
| M. Ramboux . . | " 491 " | 3 | " |
| M. Dabois . . . . | " 10,742 " | 30 | " |
| Total . . . | 47,402 | 172 | |

Average, 1 in 275½.

German practice:

| | | | |
|---|---|---|---|
| M. M. Moschner and Kursak . . . | in 12,329 labors, | 122 | times. |
| Dr. Carus . . . . | " 2,557 " | 24 | " |
| Dr. A. E. V. Siebold | " 1,003 " | 10 | " |
| Dr. E. E. V. | " 494 " | 4 | " |
| Dr. Kilian . . . . | " 9,392 " | 122 | " |
| Dr. Merrem . . . | " 157 " | 1 | " |
| Dr. Naegele . . . | " 115 " | 4 | " |
| Dr. Klugé . . . . | " 799 " | 6 | " |
| Dr. Brunetté. . . | " 100 " | 2 | " |
| Dr. Ademan . . . | " 57 " | 1 | " |
| Dr. Jansen. . . . | " 13,365 " | 15 | " |
| Total. . . . | 40,765 | 311 | |

Average, 1 in 129⅔.

It would seem strange that the larger percentage of cases occur in German practice, excelling in number those of the British and French, but why it should be so is unaccountable.

Charpentier's statistics give 577 face presentations in 139,198 confinements, or 1 in 241 cases.

Churchill's statistics give 626 face presentation in 129,489, or one in 206¾. The statistics together give us 1,203 face presentations in 268,677 labors, or 1 in 223 and a fraction of cases.

The causes of this form of presentation are as numerous as are the opinions of the different authors and writers, the obliquity of the uterus predominating as one of the essential causes. Deventer makes the following statement: If the uterus be inclined to the right side and the head or vertex be in the left occipito-iliac position, the uterine contractions taking place in the direction of the uterine axis after the membranes are ruptured, will force the fœtus from above downwards and from right to left, so that the vertex will strike against the left border of the superior strait; the head, thus arrested, will be thrown back upon the posterior plane of the child.

Baudelocque, while admitting the right uterine obliquity, believes that at the same time a right occipito-iliac position of the vertex exists.

Dr. Charles D. Meigs, the first American writer that seems to have thoroughly understood and described face presentations, writes, "The cause of face presentation is not perfectly well understood; it is, however, probable that they are more commonly occasioned by an obliquity of the womb than by any other cause."

Leishman says: "The causes which lead to this unusual occurrence are not well understood, but the initiary movement which results in the position, can only be, as is obvious, a movement of extension, which at an early stage of labor, or prior to its occurrence, is substituted for the usual movement of flexion occurring during labor in the ordinary positions of the cephalic extremity; in other words, and to take the most simple view of the matter, cranial are converted into facial positions by a simple movement of the head on its transverse axis.

It will be seen with what doubt the above theory is advanced. Be the cause or causes what they may, either simple or complex, the fact remains that we have to deal with a premature extension of the head.

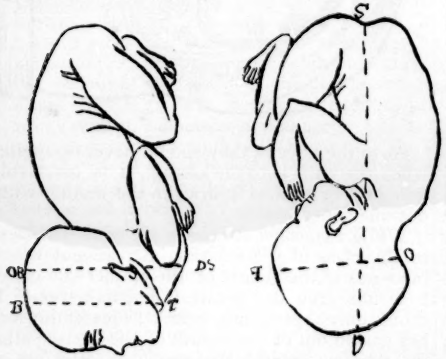
It is remarkable what diversity of opinion different writers have, in regard to the mechanism and treatment of this class of presentation. Many simply state that face presentations have the same mechanism as the vertex, the diameters are of the same dimensions and therefore must go through the pelvis in the ordinary manner of a vertex, an undoubted error. Other writers look upon a face presentation as unnatural labor demanding prompt interference. One author dismisses the subject in just 4½ lines.

In 1876, Prof. R. A. F. Penrose[1], of the Medical

[1] Penrose on the Treatment of face presentations. American Supplement to the Obstetrical Journal of Great Britain and Ireland. April, 1876.

Department of the University of Pennsylvania, was the first to accurately describe the mechanism and treatment of this form of presentation.

When we compare the mechanism of vertex and face presentation, apparently it will seem that there is no difference, but in reality a great difference exists. In vertex we have the body of the child completely flexed, the chin well thrown on the chest and the head, consequently, in the most desirable position to enter the superior strait; the full force of the contractions is received through the spinal column of the child in practically a direct line to the flexed vertex, the head reaching the floor of the pelvis, the necessary resisting force is encountered, and rotation takes place, the head passing under the arch of the pubis. Face presentations are much more difficult and much more dangerous presentations. In face presentation we have a state of extension, the chin leaving the chest and the head thrown back on the posterior plane of the child; the parturient or propelling forces, instead of passing through the spinal column of the child in a direct line, pass at a right angle; what is the result? The face instead of advancing steadily like in a vertex presentation, advances slowly and uncertainly, descends only as far as the length of the child's neck, one and a half inches, and to advance any farther, it must do so by dragging down the trunk aided by the misapplied parturient forces, increase extension to its utmost limit, we have a jamming of the presenting part, should the chin rotate anteriorly labor may eventually take place. If the chin remain presenting posteriorly, the completion of labor becomes impossible.




O B—Occipito bregmatic diam., 3½ in. T B—Treachelo bregmatic diam., 3½ in.

D S—Dorso-sternal diam., 3 in. O D—Occipito bregmatic diam., 3½ in., 6½ in.

All things equal, that is, where there exists a normal relation of diameters of the pelvis and head, my experience has been that if the chin presents posteriorly (either to the right or left) at the commencement of labor and engages in the superior strait as such, unless changed by manual interference, there remains a posterior position throughout labor.

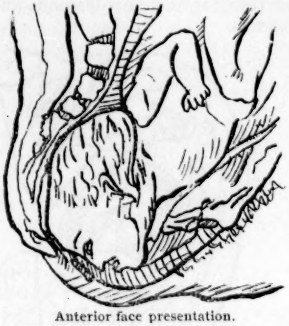
It will be remembered that the parturient forces are sent through the spinal column in a broken line, at right angle. If, as the picture below will show, the driving force is met by the unequal resistance of the two arms of the face lever. A. F., being the long lever or arm, offers more resistance than A. M., the short lever or arm, the result is, the chin descends and the forehead ascends.

Attitude of the head in face presentation. (Ribémont.)

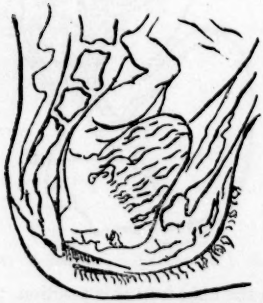
When the chin is in this position, the neck is put on its extreme tension, the extension increased to its utmost limit, it is compelled to sweep down the sacrum and coccyx, a distance of fully five inches, over the length of the perineum, which is about three inches, making in all a distance to be traversed by the chin, of eight, if not more inches, the child's neck is only one and a half inches in length, therefore before the chin can slide down the sacrum and off the anterior edge of the distended perineum, the child's thorax must be pulled and jammed down into the cavity along with the presenting face. If we study Picture No. 1, it will be seen that we have here presented, the occipito-bregmatic diameter, which is 3½ inches, with the dorso-sternal diameter of 3 inches, making together 6½ inches, to go through a diameter of 4 to the utmost 4½ inches, a radical impossibility.

In anterior face presentation, the chin, by passing under the arch of the pubis, the entire

head can be born before any part of the thorax of the child begins to enter the cavity, but no such act occurs in a posterior face; here the forehead appears first under the arch of the pubis, not acting as a pivot but firmly jammed against it. The first to escape is the chin, then the mouth, nose and eyes, top of the forehead, crown, and at last the vertex, as it will be seen, a mechanism diametrically opposed to an anterior-mento-position.



Anterior face presentation.

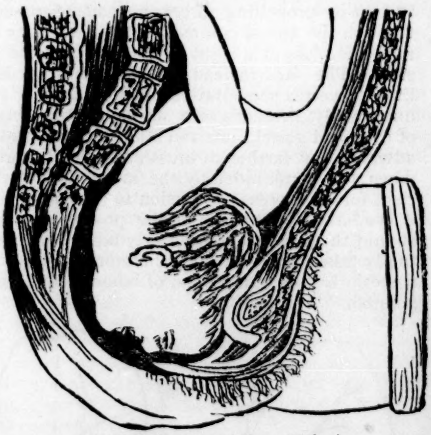


Posterior face presentation.

The treatment of mento-posterior positions practically resolves itself into two methods, version and craniotomy.

Several other methods have been suggested, prominent among which is Schatz's method. It is supposed to do away with the introduction of the hands or instruments in the maternal parts. It consists in restoring the normal attitude of the child's body by flexing the trunk and leaving the head to resume spontaneously its proper position as it sinks into the pelvis. The shoulder and breast are seized by the hand and pressed backward, at the same time the breech is raised with the disengaged hand applied near the fundus, making the long axis of the child conform to that of the uterus; then, finally, press the breech directly downward. As the child is raised, the occiput is allowed to descend, then as the body is bent forward, flexion of the head occurs by the resistance of the side walls of the pelvis. This method is fraught with the danger of the head flexion only partially occurring, leaving a brow presentation in its stead.

In regard to the use of the vectis, I have invariably failed to obtain any good results. Prof. R. A. F. Penrose,[2] who strongly advocates its use in mento-anterior positions, says: "In mento-anterior position the treatment I have described (vectis or blade of forceps to be applied to the posterior cheek of the child, rotation allowed to occur of itself, not forced) should be resorted too. If, however, the chin be turned to the posterior part of the pelvis (mento-posterior position), we have one of the gravest cases in obstetric surgery to deal with. Anterior rotation of the chin under these circumstances is possible, but so exceedingly improbable, that we have no right to expect it and we should, as soon as the difficulty is recognized, treat the case as one of impossible labor."



Posterior face presentation. (Hodge.)

As to the forceps, they should never be applied when the chin is to the sacrum, it is invariably followed by serious injuries to the mother without due compensation.

While I consider version as the main treatment in this class of presentation, a treatment which rebounds to the benefit of the mother and child, I would urge the greatest discrimination. It should only be performed before the presenting face has passed out of the mouth of the uterus; after the face has entered the pelvis all attempts at version I consider unjustifiable, for version performed under the circumstances has been known to cause laceration of the uterus and other injuries to the maternal passages. Should all attempts fail to produce a favorable change in the presenting face, craniotomy should be performed, not after hours of ineffectual labor, when the mother's

tissues are sodden and their vitality destroyed by the continuous pressure of an impacted face, she herself thoroughly exhausted, but should be done at once. The child's viability is destroyed, though alive, is practically dead, and it devolves upon us to deliver the unfortunate woman as safely and quickly as possible.

The two following cases are taken from my note book: January, 1872, I was requested by a brother physician, (who had broken his forceps in attempting to deliver a patient) to bring my craniotomy instruments. On my arrival I found the patient had been in labor since the previous Wednesday; the day of my call was Saturday. She had been at first in the care of a mid-wife, who had allowed the patient to remain in labor from Wednesday to Saturday; the family, seeing no termination, in spite of the repeated assurance of the mid-wife that everything was all right, insisted upon a physician being called. The physician, having diagnosed a posterior position of the chin, attempted to deliver with the forceps, which resulted in a broken pair of instruments. On examination, I found the chin just engaging in the superior strait, free from the mouth of the uterus, presenting in the left oblique diameter, posteriorly. I attempted to force an anterior rotation, the woman's pains being good, I held the face in the anterior-mento for fully half an hour; believing the position permanent, I withdrew my hand, examination a few moments later, revealed the chin in its original posterior position. I then used one blade of the forceps as a vectis, applying it against the posterior cheek of the child, but with no results. Craniotomy was performed and the patient delivered within twenty minutes.

In August, 1885, I was called to assist a brother physician in a case. The patient had been in labor since the previous evening. Examination revealed a posterior chin presentation, not yet out of the mouth of the uterus and just about engaging. The medical attendant, prior to my arrival, had attempted to produce an anterior rotation of the chin without avail. By the time I arrived at the bedside, the patient was exhausted and incessantly crying to be relieved. I determined to perform version at once; ether was administered and within ten minutes a living child was born. Mother and child did well.

It will be seen that in neither case had the chin or presenting part entered the cavity—that has been my experience. Mento-posterior presentations of the chin do not enter deeply into the pelvic cavity, on account of presenting at the superior strait larger diameters than could pass through any of the diameters of the strait.

221 S. 17th St., Philadelphia.

[2] Penrose on Face Presentations.

## MINOR UTERINE SURGERY.

*Read in the Section of Obstetrics and Diseases of Women, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May 1891.*

BY J. M. BALDY, M.D.,

PROFESSOR OF GYNECOLOGY IN THE PHILADELPHIA POLYCLINIC AND COLLEGE FOR GRADUATES IN MEDICINE.

In these days of major uterine and pelvic surgery, the minor operations on the uterus, of which we used to hear so much, have been in great measure lost sight of by students of the diseases of women. In consequence of this very fact of being overshadowed by newer and more alluring procedures, fortunately many of the abuses of the old days have disappeared from the practice of those working in gynecology in our large medical centers. Unfortunately this is not equally true of the vast majority of practitioners either in the cities or in the rural districts; the old teachings and the old books still manifest too much influence. The time has come when we should carefully look back over our work of the past few years and see what lessons it has taught us.

The one great fact which stands staring us in the face in this connection, is that most of the troubles which we have been in the habit of considering as situated in the uterus, are not in that organ at all. Thus with one sweep we are able to dispose of a large proportion of the so-called uterine diseases. It is then manifestly improper and dangerous to direct treatment to the uterus, when that organ is perfectly healthy and the trouble is situated in the neighboring tissues—most probably of an inflammatory nature. In a large proportion of the remaining so-called uterine diseases the womb itself is undoubtedly affected, but the lesion in the uterus is only secondary to a lesion in the uterine appendages, or if it was the seat of the primary disease, its continuance is surely due to the inflammatory condition of the contiguous organs. Here again, a treatment directed against the uterus itself is both irrational and dangerous. This leaves us but a comparatively small number of cases of pure uterine disease; such is the true situation. Uterine disease, such as metritis, endometritis, pathological displacements, etc., uncomplicated by other and more serious troubles, is the exception rather than the rule, and in a large dispensary practice during the past few years the small number of such cases which have come under observation, has been a matter of surprise to me.

It would seem, then, that the old methods proposed for the treatment of uterine diseases were not in themselves necessarily faulty, although most of them have undoubtedly been carried to excess, but that the trouble lay in faulty diagnosis and consequent application of methods of treatment unsuitable for the true disease. If the true lesion be overlooked and many of the

minor operations be performed, the result is sure to be disastrous in a certain proportion of cases. How often an acute pelvic inflammation will be lit up, will depend, in great part, on how carefully the given manipulation is carried out.

All this being true, there still remain a certain proportion of diseases, which are uterine pure and simple. The fact, that in the past so many mistakes in diagnosis have been made, and that disastrous results have followed the application of certain treatments, should not blind us to the value of these same methods in cases in which they are properly applicable; and yet at the same time a proper limit should be put to their use.

*The Uterine Sound.*—On the whole it had been better for womankind had this instrument never been invented. For the application of the elecric current it has its uses, and in an occasional case where one is uncertain as to the connection of the uterus with a given tumor, it may possibly give some definite information. For the purpose of measuring the depth of the uterine cavity or for locating the position of the fundus, as it is generally used, it is absolutely superfluous; any one who is forced to use it for these purposes, excepting in rarely exceptional cases, must confess to himself his lack of even an ordinary amount of manual dexterity. For replacing a displaced womb it is a dangerous instrument and its use is very painful. A movable uterus can be invariably replaced by bi-manual manipulation, with the patient on the back, or in the knee-chest position, with the aid of a tenaculum and a repositor. If the organ is adherent it no longer comes under the head of simple uterine disease and should then be dealt with by other and safer procedures. The instrument may be used with perfect safety in careful hands, but as ordinarily employed it is capable of causing considerable tranmatism and of carrying septic material into the uterine cavity.

*Dilatation.*—This operation is one which has been much abused. It is capable of much good or much harm, depending upon whether it is used carefully and in suitable cases, or not. Personally, I have never had any permanent bad results follow its use. On the contrary, I have seen severe dysmenorrhœa relieved and even permanently cured; in not a small proportion of cases have I observed pregnancy follow dilatation. Patients have come to me suffering from a pelvic inflammation following a dilatation, performed by others, but everything pointed to the fact that the pelvic disease had existed first and that the operation had been performed, probably under a mistaken diagnosis, for the relief of symptoms produced by the diseased appendages. In but one case have I had an inflammatory attack follow this operation when there was no complicating disease. In that case I made an incomplete dilatation without ether, upon the dispensary table, in a young woman, married for five years and sterile. The inflammatory attack cleared up within a week, she soon conceived and a month ago I delivered her safely of a large, healthy baby. She made a perfect and uncomplicated recovery. I should be exceedingly loath to discard the dilators from my armamentarium, and can only attribute serious accidents following their use, to a wrong diagnosis or a faulty use of the instrument.

*Intra-Uterine Medication.*—There is undoubtedly a limited field for the application of drugs to the uterine cavity. It should not be forgotten, however, that it is often only supplementary to other treatment. It is just as rational to expect results from local applications to the uterine mucous membrane, as from applications to the mucous membrane of the nose and throat. The fault here is much the same as in the other minor operations; either a wrong diagnosis is made, and local applications are used to the exclusion of other and probably better methods of treatment, or they have been injudiciously and rudely applied. Certainly in a chronic endometritis we may expect some good from them, especially after the application of the curette.

*The Curette.*—This instrument, in properly selected cases, is invaluable. In my own hands the dull curette has proven worthless and I much prefer a sharp one. Of course the instrument is capable of harm, but so is every other one used in surgery. If even ordinary precautions are taken in selecting cases and manipulating the curette, no damage will follow its application. For affections of the endometrium following labor, or of a fungoid character, it is exceedingly useful: I know of nothing which will take its place. In removal of retained clots, etc., in the puerperium the finger will be found to be more certain.

*Operations on the Cervix.*—The splitting up of the cervix for dysmenorrhœa and sterility has fallen into deserved disuse: all that can be accomplished in that direction, can be done with the dilators. Emmet's operation for the closure of lacerated cervix should meet much the same fate, at least in the vast majority of cases. The only excuse for the number of such operations which are being continually performed, is a fear of subsequent cancer. A simple uncomplicated cervical tear, causes no more trouble or inconvenience to the patient, than does a woman's ear which has been torn through by her earring. The ear would be repaired for cosmetic effects; the cervix is hidden from view, and that factor would not come into consideration. Most women have lacerations of the cervix of more or less degree. If the lips are thickened, everted and eroded, they will need treatment. Oftentimes simple scarification, followed by the application of iodine and glycerine tampons, will re-

duce this condition and leave a clean, healthy tear; one which will remain so until the end of time. If the lips cannot be brought into a healthy condition or if the uterus is subinvoluted and the endometrium diseased, the case may resolve itself into an operative one; but even here a trial with electricity will often disappoint the surgeon. In my practice it is rare to see a lacerated cervix which calls for surgical repair. Cases which come to me from other men, who have advised an operation, get well without it. Oftentimes the symptoms, for which the operation was proposed, were found to be due not to the torn cervix at all, but to other troubles, principally constitutional.

It is the easiest thing in the world to relight a pelvic inflammation whilst repairing a cervix, and I have seen this result follow such practice only too often.

Taking it all in all, I decidedly approve of minor uterine surgery in the field to which it is applicable, but it must be borne in mind that this field is a limited one, and one which becomes more and more narrow as our diagnostic resources increase.

---

## THE RELATION OF CONCUSSION OF THE BRAIN AND SPINAL CORD TO INFLAMMATORY AND OTHER MORBID CONDITIONS IN THESE ORGANS.

*Read in the Section of Surgery and Anatomy, at the Forty-second Annual Meeting of the American Medical Association, held at Washington, D. C., May 5, 1891.*

BY B. A. WATSON, A.M., M.D.,

CONSULTING SURGEON TO THE BAYONNE HOSPITAL, SURGEON TO JERSEY CITY HOSPITAL, ETC.

It is certainly apparent to all the members of the medical profession that brain and spinal surgery has received during the last quarter of this century an unusual and likewise unprecedented amount of attention. It is likewise equally apparent that great progress has been made by those who are devoted to this branch of surgery; it therefore becomes highly necessary, that he, who is about to enter this field of medical literature, should first discover some dark spot or disputed question, on which he thinks he may possibly throw a few rays of scientific light. The traumatisms of the brain and its membranes have been more fully studied than those of the spinal cord. Undoubtedly this may be accounted for in part by the fact, that the first mentioned organ is much more frequently the seat of these lesions than the latter. I shall therefore direct the greater portion of my attention especially to any lesions following traumatic injuries of the spinal cord and its membranes.

It seems necessary to call attention to certain errors as a preliminary to the consideration of our first inquiry. One of the most important of these has been expressed by Mr. Erichsen in the following language:[1] "The consideration of the effects that may be produced on the spinal cord by *slight blows*, whether applied to the back or to a distant part of the body, is not altogether a matter of modern surgical study arising from the prevalence of railway accidents, but had, long antecedent to the introduction of modern means of locomotion, arrested the attention of observant practitioners." Since the above statement was made it has been clearly demonstrated to be erroneous by anatomical and experimental investigations. The anatomical studies have shown that the spinal cord is the best protected organ in the body, and experimentation has fully confirmed this position. The experiments which I made on dogs—the total number being 141—which were arranged and conducted for the especial purpose of producing concussive lesion in the spinal cord and its membranes fully convinced me that it required *a very severe blow*, which must even be delivered on certain limited areas of the body, in order to accomplish this object. I am likewise satisfied that this conclusion must necessarily follow a careful study of my report entitled "An Experimental Study of Lesions Arising from Severe Concussions."[2]

It should be furthermore stated that the lesions produced in the spinal cord and its membranes were rarely apparent to the unaided eye, but were readily revealed under the microscope. This statement, however, does not apply to those cases in which there were either fractures or dislocations involving the vertebræ; since the lesions here were comparatively coarse.

The statement that a rupture of the spinal membranes has occurred, or that there has been a gross hæmorrhage into the substance of the spinal cord, or between the cord and its membranes, is certainly not entitled to credence unless coupled with the acknowledgment of either a fracture or a dislocation of a vertebræ. This statement is based on my examination of the lesions found in the spinal cord, and its membranes, and I am so thoroughly convinced on this point, that I feel fully warranted in making this positive statement. The so-called "railway spine" includes true concussion arising from blows, falls, gunshot wounds, lightning strokes, electric shocks, etc., likewise contusions on the back, sudden flexions of the spinal column, sprains, wrenches, and twists. Dr. John A. Lidell says, in discussing the consecutive effects of some of these injuries, that:[3] "Among the remote effects of *sprains*, *wrenches* and *twists* of the spinal column, are chronic *inflammation of the vertebral*

[1] On Concussion of the Spine. A New and Revised Edition. London: Longmans, Green & Co. 1882.

[2] P. Blakiston, Son & Co., Philadelphia, 1890.

*joints* that are implicated, which is, often suppurative in character, *destruction of the articular cartilages* and *the intervertebral substances* that are involved, and *caries* or *necrosis* of the adjoining vertebral bodies." In such cases, the destructive process begins more frequently at the junction of the vertebræ with the intervertebral substances than in the intervertebral substances or vertebræ themselves, because, as Mr. Hilton has pointed out, we know that in accidents, at least as far as we have been able to discover, "the most frequent lesion in injury to the spine is a partial severance of the vertebræ from the intervertebral substance."

A number of illustrative examples have already been presented, and inasmuch as this topic has already been pretty thoroughly discussed, no apparent need exists for presenting any additional instances of the same sort. I will, however, take space to present a very instructive case, in which there simultaneously occurred lumbo sacral abscess and suppurative spinal meningitis, in consequence of a blow on the spinal column.

The following abstract made from the report of the history of this case supplies the essential points: A healthy lad, of 15 years, was struck by his playmate on the back with the first. He thought little of it at the time, but was admitted to Guy's hospital nine days afterwards; was treated with leeches and somewhat recovered, but the pain soon returned and fever ensued. "An abscess formed on the right side of the sacrum, which was opened, and continued to discharge, the flow of pus being increased by pressure on the abdomen. He continued to get worse daily, having much irritative fever and severe pain in the back. During the last week of his life he was exceedingly restless, and often delirious; and he complained of pain in all parts of his body, but particularly in the extremities. On one or two occasions he had loss of power over the bladder and rectum, but had no other symptoms of paraplegia, and could move freely in bed." He died twenty-two days after the casualty.

*Autopsy.*—An aperture in the integuments at the right side of the sacrum led into a very extensive abscess, external to the peritoneum, which occupied the forepart of the sacrum behind the rectum, and external to the ilia on both sides behind the psoas muscles. The bones were exposed but not diseased. Although the abscess had discharged externally on the right, it was most extensive on the left. It had burrowed up to the left side of the last lumbar vertebra and the sacro-vertebral foramen into the spinal canal. When the theca was opened, it was found to contain a quantity of greenish pus, spread over its inner surface and over the cord itself. The spinal dura mater (theca) at the point indicated, was softened and destroyed, and the cauda equina was lying bathed in pus which filled the sacral canal. The membranes of the cord were inflamed throughout the whole extent, and there was purulent effusion as high as the dorsal region. The spinal dura mater was thickened, and its inner surface had lost its smoothness and transparency, and was of a dull green color. Pus could be squeezed out from beneath the visceral arachnoid in considerable quantity. The spinal cord itself was firm, and the microscope revealed no morbid condition in its substance. On opening the cranium, traces of acute arachnitis were found over the whole surface of the brain, greenish-colored lymph being effused into the sub-arachnoid tissue, especially at the base. The inner surface of the dura mater, around the foramen magnum and on the adjacent part of the occipital fossa, was of a greenish color, from lymph effused upon it. Bronchial tubes filled with tenacious mucus. Lumbar and bronchial glands slightly enlarged. All other organs entirely healthy.

The purulent infiltration of the spinal meninges which was observed in this case, occurring coincidently with the formation of a lumbar-sacral abscess, but without the production of paralysis, could scarcely have happened unless the sacro-vertebral articulation had previously been opened, both externally and internally, by disease of the articulation itself, in such a manner as to allow the products of inflammatory action to flow freely out of it, as well as into the spinal canal. Otherwise the suppurative meningitis would pretty certainly have caused paralysis by compressing the spinal cord with its inflammatory products.

The clinical history of this lad's case, interpreted by the post-mortem appearances, appears to have been as follows: The blow on the back wrenched the sacro-vertebral articulation, and caused a suppurative inflammation to be lighted up therein, particularly on the left side; and in consequence of this, purulent matter escaped in an outward direction, and led to the formation of an immense lumbo-sacral abscess; it likewise escaped in an inward direction, and caused the theca vertebralis to become softened and perforated, and extensively destroyed, and a diffuse suppurative inflammation, which extended upward to the brain, to be kindled in the spinal arachnoid.

It has been aptly said that "in all science error precedes the truth, and it is better it should be first than last."[4] I have cited the case reported by Dr. Lidell, with all its details, in order that I might bring forward some of the results of my observations, made during my recent experiments, which afford a correct and rational explanation of the conditions revealed by that post-

[3] International Encyclopædia of Surgery, Vol. IV, p. 870 et seq. Edited by John Ashhurst, Jr., M.D., Wm. Wood & Co., New York. 1884.

[4] Walpole.

mortem examination. There will be found in the report of my experiments eleven cases in which there was rupture of the ligaments of the sacro-lumbar articulation. There were marked pathological changes in the organs and tissues within the pelvic and abdominal cavities in six of these cases, which were immediately connected with the rupture of the ligaments of the sacro-lumbar articulation. Let me here present the following abstracts from post-mortem examinations, for the purpose of giving a correct idea of the lesions:[5] "Autopsy. This examination revealed a rupture of the ligaments between the last lumbar and the first sacral vertebræ, deep and extensive ecchymoses over the entire post-sacral and right lumbar regions. A profuse extravasation of blood into the abdominal cavity, marked engorgement of the kidneys. Brain apparently normal, slight increase of the cerebro-spinal fluid in the cavities, and the cord was congested, to the middle of the dorsal region."

"Autopsy.[6] This examination revealed the following lesions: Both kidneys congested; ecchymosis in the connective tissue covering the anterior surface of the last lumbar and the upper sacral vertebræ. The brain and spinal cord apparently hyperæmic. The removal of the integument over the sacral and lumbar regions showed an ecchymotic condition of the muscles. There had been a rupture of ligaments between the last lumbar and upper sacral vertebræ."

"Autopsy.[7] This examination revealed the following lesions: Both kidneys congested; bladder distended with urine; extensive ecchymoses in the lumbar and pelvic regions, involving psoas muscles; brain perfectly normal. The spinal cord was hyperæmic downward from the lumbar enlargement, including a portion of the caudia equina; rupture of the ligaments between the last lumbar and the first sacral vertebræ." The essential point which Dr. Lidell has overlooked in his comments on the case which he has reported, are the pathological conditions of the organs and tissues within the pelvic and abdominal cavities. It was the *existance of these lesions which gave rise to the inflammation and suppuration* within these cavities. The post-mortem examination of Dr. Lidell's case sufficiently demonstrates this position. Dr. Lidell says:[8] "the blow on the back wrenched the sacro-vertebral articulation, and caused a suppurative inflammation to be started up there." The autopsy in the case fails to show that there is any erosion of the bones or cartilage within this articulation, and its history reveals the fact, that it was only immediately before death that there was the slightest indication of paraplegia. It is therefore certain that the wrench which he mentions consisted in producing a rupture of the ligaments of the sacro-lumbar articulation, and likewise, as very frequently happens in those cases, some pathological lesions within the pelvic and abdominal cavities. The simple wrenching of any joint in a healthy subject unattended with pathological lesions, has been amply shown to be a comparatively trivial injury, which does not lead to serious inflammatory complications or death.

The one hundred and forty-one experiments, which I made on animals, does not afford a *single example* which justifies the conclusion that injuries to the vertebral articulations are a whit more serious than those produced in the carpal and tarsal articulations; and, consequently, analagous reasonings may be here employed with entire propriety. We cannot pass over the history of Dr. Lidell's case without pointing out a glaring absurdity, which may possibly have had its origin in carelessness. In this report we are informed that a healthy boy, aged fifteen, "while playing with another lad, received a blow on the back with the fist," and in the absence of any additional statement bearing on this injury, we are left to infer that the pathological changes and even the death of the patient were directly caused by this blow. He who has examined the anatomical structure of the spinal column will readily perceive the absurdity of this statement. The blow *inflicted was entirely inadequate to the production of the results* which followed, unless there has been omitted from this report the most important factor in the history of this case. It is true the force of the blow might cause the lad to fall—and if at this point, the history of the case showed that the boy fell across a bar, raised a few feet from the ground, in such a manner that his feet remained on one side while his head was on the other, then the very important factor of leverage having been added, which would result under these circumstances in the production of a sudden flexion of the spinal column, would certainly bring within the limits of possibility the occurrence of the injuries which are reported in the autopsy.

It is scarcely necessary to mention the fact, that nearly all the injuries grouped together under the unfortunate cognomen of "railway spine"—so far as their remote effects are concerned—take their origin, either directly or indirectly, in inflammatory processes. Consequently the consideration of the following queries assume importance:

1. Do inflammatory and other morbid changes take their origin in traumatic injuries of the spinal cord and its membranes as frequently as the writings of Mr. Erichsen imply?

2. Do inflammatory and other morbid changes ever arise from traumatic injuries which are entirely uncomplicated by immediate symptoms?

[5] An Experimental Study of Lesions Arising from Severe Concussions," by B. A. Watson, A.M., M.D., p. 32, Exp. 4.

[6] Ib., p. 35, Exp. 10.

[7] Ib., page 40, Exp. 24.

[8] Previously cited.

3. How soon are the inflammatory and other morbid changes developed after the receipt of traumatic injuries in the cord, etc.?

In our consideration of the first question it is scarcely necessary to give a negative reply to those who have perused the writings of Mr. Erichsen on this subject; since they have already reached this conclusion. Furthermore, the mere assumption that serious results frequently arise from wholly unimportant injuries *is not sustained* by clinical observations or experimental investigations. However, Mr. Erichsen, in writing of those cases in which the patient has received no blow or injury upon the head or spine, but in which the whole system has received a severe shake or shock, in consequence of which an immediate lesion, probably of a molecular character, is sustained by the spinal cord, and disease of an inflammatory character, or of a disorganizing nature, is developed in it, the inflammatory action eventually creeping up to the membranes of the brain, says that the cases [9] "although *necessarily more frequent* in railway than other injuries, do occasionally occur as a consequence of ordinary accidents." A perusal of Mr. Erichsen's writings has completely failed to convince me that he has supplied any basis for the support of the opinion advanced in the above statement.

In order to demonstrate the correctness of this opinion, which conflicts with the positive knowledge we possess on this subject—and which cannot be supported by analogous reasoning—he should clearly establish the following facts, viz.: 1. *That no disease of the brain or spinal cord existed at the time of the occurrence of the alleged accident.* 2. *That the consecutive inflammation did arise from traumatism, and was not due in part, or wholly, to a vitiated constitution.* This evidence is entirely wanting; and, consequently, we must maintain that Mr. Erichsen's statement should be excluded in cases of medico-legal evidence.

We have now devoted our attention to the consideration of the material points involved in our first query; but it would seem proper in this connection to give some additional thought to the consecutive results of concussive accidents. Mr. Erichsen says:[10] "Whatever may be the nature of the primary change that is produced in the spinal cord by a concussion, the secondary effects are clearly of an inflammatory character, and identical with those dependent on chronic meningitis of the cord and sub acute myelitis." The term concussion, as employed in the above quotation, evidently refers only to traumatism of the spinal cord and its membranes, but elsewhere the same author in speaking of sprains, twists and wrenches of the spinal column, says,[11] "It is important to bear in mind that the vertebral column is more apt to suffer in these strains of the spine than in the other forms of injury that we are discussing, and that in serious cases the full force of the mischief appears to be expended in the spine itself independently of its contents, which escape uninjured." The views expressed by Mr. Herbert Page on this subject are as follows:[12] "Happily there is no doubt of the exceeding rarity of spinal meningitis as an immediate result of localized injury to the vertebral column; and I know of no case in which meningeal inflammation has been caused by injury of some part of the body remote from the spine." The experimental study of concussion of the spinal cord has satisfied me on the following points: 1. True concussion of the spinal cord from blows, falls, lightning strokes, etc., is an exceedingly rare occurrence. 2. Secondary results, such as inflammation, suppuration, etc., very seldom follow the traumatism—owing to the fact—that the punctate hæmorrhages, etc., are generally widely diffused in various parts of the spinal cord—while the pathological lesions are so slight as not to supply inflammatory foci. However, the danger arising from a true concussion of the spinal cord produced by gun-shot injuries, the lodgment of a musket or rifle ball in the spinal column, is attended with much more danger from consecutive inflammation, etc., than those traumatisms which have been previously mentioned, because of the following peculiarities: 1. The pathological lesions are more concentrated—resembling in this respect the ecchymoses arising from a concentrated contusion. 2. The lodgment of a ball in the spinal column is very liable to give rise to a septic inflammation, pus formation, diseased condition of bone, extension of the inflammatory products to the spinal membranes, and even the cord itself. It must be universally admitted that gunshot wounds, fractures and dislocations involving the spinal column, are very serious traumatisms, frequently producing death immediately, or within a few hours; even when this result does not follow, the danger is very great from the consecutive sequelæ.

Let us now take up the consideration of the second query: "Do inflammatory and other morbid changes ever arise from traumatic injuries which are entirely unaccompanied by immediate symptoms?" The conclusions reached by a careful analysis of the above question, must depend, to some extent, on the interpretation which we give to this query. The fact must be admitted that it is not only possible for a patient to receive, under certain circumstances, severe traumatic injuries, of which he remains a longer or shorter period wholly unconscious, and these occurrences are by no means very rare. I can

[9] On Concussion of the Spine, Nervous Shock, etc., p. 95. New York: Wm. Wood & Co. 1875.
[10] Ib., page 157.
[11] Ib., page 128.
[12] Railway Injuries, page 21. Philadelphia: P. Blakiston, Son & Co. 1891.

now recall a number of instances of this character occurring to officers and soldiers during the excitement of battle, while I was serving with the army. It also occasionally happens that drunkenness dulls the sensibilities in certain cases to such an extent as to render the patient oblivious to an injury which he may have received, while the same condition will more or less embarass and sometimes even deceive the surgeon who is attempting to make an examination. These statements possess only an indirect bearing on the question under consideration. The question relates to the immediate existence of symptoms and not to the observance of the same. A critical examination of this question in the same direction indicated in the above sentences, brings us to the following query: "Are results ever produced without causes?" *i. e.*, "Do inflammatory and other morbid changes ever arise from traumatic injuries, which have failed to produce any lesions?" My experimental studies of the lesions of the cerebro-spinal axis have clearly demonstrated the fact, that pathological changes are occasionally produced in this centre, some of which were even apparent to the unaided eye, while others required the aid of the microscope; but which were not followed by any symptom indicative of these lesions during the life of the animal. The question will now be naturally asked with reference to these cases: Would they not have developed inflammatory changes at a later period? I have not before me the data which would justify me in positively asserting that a result *was absolutely impossible;* but I can aver that, in all these experiments, one hundred and forty one, there is not *a single* case which *warrants giving an affirmative answer.* Surgical observations and analogous reasoning fully support the results obtained by our experimental research, not only with reference to the brain and spinal cord, but likewise in other organs of the body. Mr. Erichsen says in his recent work "On Concussion of the Spine:"[13] "There is a continuous chain of broken ill health, between the time of the occurrence of the accident and the development of the more serious symptoms. It is this that enables the surgeon to connect the two in the relation of cause and effect. This is not peculiar to railway injuries, but occurs in all cases of progressive paralysis after spinal concussion." It would appear from this quotation that Mr. Erichsen fully concurs with us in the opinion, that the production of a traumatic pathological lesion is always indicated by immediate symptoms, when the degree of injury has been sufficient to entitle it to our consideration. Observation has taught us that the existence of a pathological lesion affords insufficient proof that this injury will be followed by inflammation or other morbid changes. In fact, every surgeon can recall to mind cases of severe traumatic injuries of the brain in which there were extensive lacerations or contusion of this organ; but in which the secondary changes were entirely absent or very slight.

The same remark may also be made in regard to gross injuries in other parts, since the same general physiological and pathological principles prevail in the cerebro-spinal axis as in the other organs of the body. It is therefore entirely proper to introduce in connection with discussions of injuries of the cerebro-spinal axis, in many cases, analogous reasoning drawn from studies and observations made in other parts of the body. It must be admitted, as a general rule in surgery, that the more extensive and the more destructive the primary traumatism, the greater will be the danger from secondary disorganizing processes. Does it not, therefore, seem absurd to attribute to a single punctate hæmorrhage, or even a half dozen of these little pathological lesions, the power of exciting a disorganizing process in *a healthy spinal cord* which may ultimately produce the death of the patient? Is there a single well authenticated case in which such a result has followed from such a cause?

Let us now proceed to the consideration of our third question, which has a very important bearing on medico-legal cases and likewise on the diagnosis and treatment of myelitis arising from syphilitic disease. Mr. Erichsen says:[14] "It would appear that surgeons and writers on diseases of the nervous system have included four distinct pathological conditions under this one one term, 'concussion of the spine,' viz.: 1. A jar or shake of the cord, disordering, to a greater or less degree, its functions without any lesion perceptible to the unaided eye. 2. Compression of the cord slowly produced by the extravasation of blood. 3. Compression of the cord by inflammatory exudations, serum, lymph, or pus within the spinal canal; and 4, chronic alterations of the structure of the cord itself as the result of impairment of nutrition consequent on the occurrence of one or other of the preceding pathological states, but chiefly on the third."

We have already paid our respects in full to those subjects included under the headings 1 and 2, but we have yet to deal with those embraced under headings 3 and 4.

There exists no reason to doubt that inflammation, having its origin in the spinal cord or its membranes, may be either acute or sub-acute, or that the former need be developed at an earlier day than the latter. The important question before us for our consideration, however, is how soon do traumatic inflammations arise after the receipt of the traumatism? This question can

[13] A new and Revised Edition, page 158. London: Longmans, Green & Co., 1882.

[14] On Concussion of the Spine, p. 16. London: Longmans, Green & Co., 1882.

not be definitely answered—the answer must necessarily be a mere approximation. Nevertheless, I think that observation and experimentation fully justify the statement that traumatic inflammation commonly makes its appearance within four days after the receipt of the injury. In support of the above I will here present a series of experiments, which consisted in fracturing the right leg of twenty rabbits:[15] "This was done without producing a single compound fracture. The temperature details were as follows: Average temperature before the fracture, 102.6°; average temperature two hours afterwards, 102.6°; subsequent daily average temperature, first, 102.6°; second, 102.6°; third, 103.1°; fourth, 103.1°; sixth, 103°; eighth, 103.1°; tenth, 102.8°; twelfth, 103°; fifteenth, 102.8°; seventeenth, 103.2°; nineteenth, 103°; twenty-second, 103°."

In the case of the brain and spinal cord, when there is no open wound communication with these organs, the danger of inflammatory action is greatly diminished. Furthermore, the trivial character of lesions produced by concussion, *and also the exceeding rarity of this traumatism*—except when it is produced by lightning strokes, electric shocks or gunshot wounds—all combine to impress me with grave doubts in regard to its being followed by inflammation. In fact, *there is not sufficient proof to justify the conclusion* that any case of traumatic myelitis, or meningo-myelitis, has ever had its origin from molecular disturbances or any pathological lesion which was invisible to the unaided eye in an otherwise healthy spinal cord or its membranes.

Let us now briefly examine the history of concussion, in order that we may, if possible, discover how these troublesome errors were engrafted on the profession. The history of concussion dates from 1705, when M. Littre published his famous observations. A prisoner struck his bowed head against the wall of his cell and fell dead. The inspection of the head failed to reveal a contusion, tumor or wound in the scalp, or a fracture of the skull. The cranium was opened, when it was discovered that the brain did not nearly fill the interior of the skull, while the entire brain substance seemed harder to the touch—more compact than usual. This observation has been reported hundreds of times, and it was long claimed that it demonstrated the fact that a violent shaking of the substance of the brain was capable of producing death without any appreciable lesion. Permit me here to call attention to the fact that in the report before me there is no evidence to show that there was any post-mortem examination made of any portion of the spinal column, spinal cord or its membranes, or other portion of the body, except the head. The autopsy of the head, viewed in the light of modern science, is entirely worthless, while the position in which the head was placed at the moment the injury was inflicted suggests the possibility of a fracture or dislocation in some portion of the cervical region of the spine. It may be well to call attention to the fact that in this case, and all the other cases of concussion reported prior to the middle of the present century, the microscope was not employed to aid in the discovery of the pathological lesions. Post-mortem examinations were very rarely made even in cases of concussion of the brain, and, consequently, the case reported by M. Littre was accepted as a typical one. M. Littre, at the time he reported this case of so-called concussion of the brain, had become famous in the medical profession and therefore the following may be very properly applied to him: "Great errors seldom originate but with men of great minds."—Petrarch. Mr. Erichsen, in writing on concussion of the spine, in 1875, remarks:[16] "There is little opportunity, reason, or excuse for a post-mortem examination of that structure, which is probably the one that is least frequently examined in the dead-house, viz., the spinal cord, as it is the one the correct pathological investigation of which is attended by more difficulties than that of any other organ of the body. . . . . So rare are post-mortem examinations of these cases that no instance has occurred to me in hospital or in private practice in which I could obtain one, and with one exception, I can find no record in the transactions of societies or in the periodical literature of the day of any such instance. The only case, indeed, on record with which I am acquainted, in which a post-mortem examination has been made of the spinal cord of a person who had actually died from the remote effects of concussion of the spine from a railway collision, is one that was published in the 'Transactions of the Pathological Society,' by Dr. Lockhart Clark. He had been in a railway collision, and without any sign of external injury, fracture, dislocation, wound, or bruise, began to manifest the usual nervous symptoms. He gradually became partially paralyzed in the lower extremities, and died three years and a half after the accident." There is no reason to believe that this man died from even the remote effects of the traumatism. The history of this case entirely fails to connect the disease from which the patient died with the railway collision. Therefore it may be properly said of those who base an argument on this and all similar cases:

> "When people once are in the wrong,
> Each line they add is much too long;
> Who fastest walks, but walks astray,
> Is only farthest from his way."
>
> —*Prior.*

[15] Amputations and their Complications, p. 525. By B. A. Watson, M.D. Philadelphia: P. Blakiston, Son & Co., 1885.

[16] Concussion of the Spine, Nervous Shock, etc., p. 177 et seq. New York: Wm. Wood & Co., 1875.

## TUBERCULIN, THE VALUE AND LIMITATIONS OF ITS USE IN CONSUMPTION.

*Read before the Colorado State Medical Society, June 17, 1891.*

BY CHARLES DENISON, A.M., M.D.,
OF DENVER, COLORADO.

I assume that no apology is necessary for my changing the subject of this paper from the Surgical Treatment of Lung Cavities to the discussion of Koch's lymph; because of late my mind and opportunities have been turned in the latter direction. Besides, my professional brothers expect some report of my investigations of the great German scientist's new remedy, and now and here are the best time and place for it. Now, because time enough has elapsed for the expression of a great variety of opinions, and here, because no *symposium* on consumption is complete without a consideration of this recent invention for its amelioration.

The questions which to my mind need most to be answered are,

1. Was the description given us of the composition of Koch's lymph sufficient for our gaining full knowledge as to its uses?

2. Were the rules laid down by the author adequate and complete enough for the use of so powerful an agent?

3. Is the knowledge of tuberculosis sufficient, or are the means and facility of its diagnosis adequate among the average of medical men for their use of tuberculin, as proposed by Koch, without a considerable chance of damaging the patients, as much as they would benefit them?

I have arranged these questions purposely that I might answer them as I do, with a decided No. It must then follow that this is not yet a remedy to be indiscriminately sold by druggists, and generally used by all sorts of doctors.

The much to be desired time will come, if this remedy is not too soon consigned to oblivion, as it was at first placed on the top of the Mountain of Hope, that the knowledge of tuberculin and of tuberculosis likewise, will be adequate to satisfy a critical mind. But that time is not yet.

Nevertheless, with all these doubts and suggested limitations, the use of tuberculin, though unconsciously faulty, is not to be forbidden, if we can show that we are running only a ten to twenty per cent. chance of danger for the purpose of a thirty to sixty per cent. chance of benefit. The reason for this discussion is thus apparent, that through the process of exclusion, conditions only favorable to the use of tuberculin may obtain, and the percentage of danger be reduced to the minimum.

This criticism of the German professor's methods must not be considered as an under-valuation of that scientist's true worth. Robert Koch stands to-day without a living peer in the great and growing profession of medicine, for he has originated more for scientific advance than any two men living. He has not only discovered the bacillus of tubercle so that physicians all over the world could easily demonstrate it as an essential feature in every tuberculosis, but in a few years thereafter he has produced a fluid which wars against the *habitat* of that germ, thus tending to eliminate its consequent disease. When we reflect that tuberculosis exterminates more of the human race than any other disease under the sun—that is, ever since Jenner's vaccine virus "knocked out" small-pox,—we say all honor to Robert Koch, the greatest physician of the age.

We would, if possible, find out the inadequacies or mistakes in Prof. Koch's procedure, that the perfection of his great discovery might be brought nearer to hand. For I believe that rightly directed criticism will do much to clear away the doubt and prejudice which largely pervades the professional mind as to this means of cure.

Let me then consider the questions previously propounded, and see why they should be answered in the negative. I will use the cases I have experimented on for purposes of corroboration or reference, and try not to burden your minds or patience too much with their many details. We shall confine our attention wholly to pulmonary cases. These cases have been twenty in number. Numbers one to five of these most people would call cured. I do not yet claim that term as applicable, yet they are probably better off than in a state of arrest, because the tuberculosis has been in a process of elimination. These twenty cases were serious enough; distinctly tubercular, yet only two of them acutely so.

Nos. 1, 2 and 3 were third stage cases, with both lungs more or less affected, while Nos. 2 and 3 had the dreaded laryngical complication.

No. 4 is interesting, as showing the diagnostic power of tuberculin. This was a case of fibrosis chiefly, evidently tubercular when he came to Colorado for his health three years previous. In March, 1891, his disease was apparently fully arrested, through the fibroid process and rarefied air. The question then arose, could he go back East to live? I obtained from his parents permission to use the tuberculin to learn if latent tuberculosis remained. I was much surprised to find a considerable local reaction in the right infra-scapular space, where previous to the first injection I had not noticed anything abnormal. This reaction was over an area two by three or more inches, and was accompanied by the characteristic febrile rise to 101°, and the appearance of bacilli in his sputum, (four to the field) which had not been found by a previous microscopic examination. This reaction, which was marked after 2, 4 and 6 mgs., wore off with the lessening of fever as larger doses were reached. Finally there was no reaction after 20 mgs., and I con-

sented to his return to Massachusetts after a 30 mg. dose and about six weeks treatment. The characteristic rough breathing in the rear base of the right lung had moderated, assuming the slightly bronchial character usual in high altitudes in an arrested fibroid lung.

*Case 5*, of my list, is to me very interesting, because it shows the undoubted agency of climate and lung ventilation in the cure, as well as the rapidity and ease with which his system took the tuberculin. Mr. F. T. P., age 27, a lawyer from Toledo, Ohio. In January, 1890, had had la grippe, followed by weakness, night sweats, cough and loss of 15 pounds in weight. In March went to Asheville, N. C. Was thin and had decided P. M. hectic there for some time. Began to gain flesh in October. Arrived in Colorado October 18th, weight at least in summer 133 pounds. October 20, 150 pounds. Temperature ranged between 98° morning and 100° evening. Spirometrical record 240. Manometer 85 M.M. Expansion 33 and 36. Sputum yellow, one-half cup chiefly mornings. Depression, moist râles on coughing, and almost "cracked metal" on stethoscopic percussion below right clavicle. Waivey and broncho. Versicular respiration in left infra-mammary and lower axillary. All showing softening (2d stage) at right apex and arrested fibroid process at base of left lung.

May 7, after spending the winter, much out of doors, in Colorado Springs, all the physical signs were found to be improved. Expectoration less one-third cup in 24 hours. Expansion increased 1 inch, 32½ and 36½. Fewer râles at right apex. Bacilli in sputum, some slight, probably compensative. emphysema in lower half right lung.

May 15, began tuberculin injections. Bacilli five to field, no general reactions (*i. e.*, no fever), but slight local reactions shown by physical signs, so a rapid increase of dose was allowable, *i. e.*, nearly doubling every other day.

May 23, after 10 mg., thirty bacilli to field.

June 2, after 45 mg., three or four scattered bacilli to the field, but still with slight remaining indications of the effect of the tuberculin local reaction, the characteristic harsh breathing in right infra-clavicular and left infra-scapular regions. Pulse, respiration and temperature normal, expansion 32½ and 37 inches. He declared he was well, and was bound to go home, to Toledo. I could not restrain him, and so with a parting 50 mg. "shot," let him go, with the advice that he return, work up to 80 mg., and then thoroughly establish his cure by camping in the mountains during the hot months of the summer. We shall see. A letter just received from him says he does not feel as well down in Ohio, and he may return. These five cases have all lost the usual signs of tubercular state, have increased in vigor and no longer react to large doses of tuberculin, 30 to 50 mg. Their sputum, diminished to the minimum in quantity, shows only the remnant of the vanquished bacillary army which was making war on the organism, at the commencement of this treatment, four months ago.

Numbers 6, 7, and 8 are now under treatment, and will soon arrive at the favorable state of those previously mentioned. They are progressing evenly and finely. These are, No. 6 a lady from Greeley, with broncho-fibro-tuberculosis, having a very small softened or bronchietic spot in each lung, now completing a mid-way rest in the treatment at 30 mg. (Since reached 60 mg.)

No. 7, a tolerated third stage fibro-tubercular case, with large cavity in left apex, who has slowly reached 7 mg.; and No. 8, a more decidedly tubercular fibroid case, both lungs affected, with a bronchiectasis in the left and a softened spot on the right, and considerable dissemination of the fibroid process. He is now getting used to 1 and 2 mg. One mg. tuberculin was too much for this patient at first, and four lesser doses had to be given before the reaction satisfied me as sufficiently mild. Nos. 9 and 10, lately commenced a cautious or tentative plan of treatment, and are third stage cases progressing satisfactorily, but the final result is not yet in sight. No. 9 is a case of laryngeal phthisis, and a process of healing can already be noticed in a tubercular ulceration on the left side of the pharynx.

Nos. 11 to 14 inclusive have ceased treatment temporarily for reasons given. They were favorable cases, however, and reached an average of 10 mg. for last treatment. These four were all third stage cases, the evidence of that advance being very positive with large cavity in one case, much less positive, and in a small space in the others. I cannot consistently, with my notions of truth, classify the consumptives we have to deal with in Colorado and put many in the second stage. If softening has occurred that is usually the end of it, and it is the result in the third stage with which we have to deal.

Of these four, Dr. ——, of Idaho Springs, reached 14 mg. and reacted slightly at that increase; lost his fever, and gained in strength and physical signs. The effect of the tuberculin in shrinking of affected lung tissue was manifest in his case. The highest reaction was 102° F. after 4 mg. March 8, after he returned home to his practice, he continued every other day 8 to 10 mg. injections himself. March 26, he wrote that his temperature had been above normal but one day, and that was due to extraneous causes and not to injections. He says he took the injections regularly "without experiencing the slightest reaction or discomfort. So far as symptoms go I feel as well as I ever did. Think my cough is about the same, but expectoration less. Am sure the expectoration has lost much of its puru-

lent character. Am still taking the tuberculin . . . . On the whole I feel that I am deriving benefit from the treatment, and would like to continue with increased dose if you see fit."

*Case 12.*—N. J. F., a laborer, aged 30. Case of tremendous cavity in left lung (see chart), reaching from clavicle to opposite left nipple, and lying on the anterior border of the lung. The regularity of his pulse 88, respiration 32, temperature 98° to 100⅘°, and my confidence in his unexcitable, steady temperament, led me to give way to his determination to try the new "lymph" treatment. I was from March 13 to April 9 reaching 9 mg., and on April 26, reached the highest point, 18 mg., when a rest of ten days was taken. Then I returned to 10 mg. at a dose, and was cautiously increasing from that point with favorable effect, when he was offered an opportunity to take charge of a squad of men working on a railroad. I believed that climate, outdoor occupation and time were as important for him as the remaining chance of benefit from tuberculin injection, and so readily consented to the change. His was, however, an interesting and exceptional case in more than one particular. His principal reaction came after the 6 mg. dose, when my attention was attracted to the characteristic rough respiration in the region of the *right* nipple, where before no abnormal sound was heard. This I have come to consider as symptomatic of the working of tuberculin in affected tissue, and is an additional diagnostic evidence which physical diagnosis alone cannot impart. Another interesting feature of this case was the seeming contraction, as the treatment progressed, of the large cavity in the left lung, due to the shrinkage of the surrounding, and probably tubercular, affected tissues. My note of the fourth recorded examination, April 23, reads, "The case has gotten along very evenly, and a marked absence of tubercular change is manifest in an even average of pulse 94, respiration 26, and temperature 98½°. The cavity is, if anything, less in size, and moved upward and toward the front. The lung, while somewhat inactive below and behind, shows only a few coarse râles. This closing up tendency of the lymph when acting between lung tissue and the main bronchus, is particularly applicable to the next case, No. 13, Rev. J. T. S., age 37, who had, he says, a contracted left lung, even in childhood, hæmorrhage 7 years ago, pleurisy, 6 years ago, and la grippe, winter 1889–1890.

My diagnosis of his case, April 1, when I first saw him, was fibroid phthisis, probably third stage, left (because of a small honey-combed spot under left clavicle), and first stage right. I have no doubt the poor ventilation marked on his chart at the second examination, April 6, over the lower half of the left lung, the air moving therein only a little except by forced breathing, was in no small degree due to the exaggerated reactions from the tuberculin injections which were manifest around the root of the left lung. I can conceive of tuberculin having this effect in a lung elsewhere unaffected, except around the largest bronchi. The previously contracted condition of this left lung in question aggravated the effect; and headache and chilly feelings were added to the usual evidences of a decided reaction, even to as little as 3 or 4 mg. I deemed it better, therefore, that measures should be inaugurated, chiefly left arm exercise, riding horseback, etc., which would tend to improve the use of this left lung, before proceeding farther with the Koch treatment.

No. 14 of the series, the wife of a prominent Chicago real estate owner, was also of special interest, but for reasons I have not seen noted by any other observer. The highest point reached in fifteen tuberculin injections was 9 mg., yet all decided reactions were accompanied by such temporary depression of spirits, with a chilliness, the second day afterward (she took them every three days), that a cessation was deemed advisable. She said at one time while reaction was going on, she cried four hours, she felt "so blue," yet the appetite was increased, as was the ability to exercise, and the local effects were not at all unfavorable. Curiously enough her feeling of depression of spirits while on board ship at sea had been just the same.

The other six cases need also to have each a separate reference, in order to understand why eventually each should be classified under the doubtful cases for completing the tuberculin treatment. Every serious case has some prominent feature about it which distinguishes that particular one from a thousand others.

*Case 15*, age 36, brick mason from Massachusetts; came with extension fibroid process in left lung, probably caused by or following tubercular softening. This process increased and was made permanent, at different times after arrival in Colorado, by typhoid fever, pneumonia and by pleurisy. The strength of the fibroid tissue was shown by his pressure record on the manometer to be the normal for a man in health, 100 mm., while his spirometrical record was only 112 cubic inches, less than one-half of what it should be for a healthy man of his height. Nervous temperament, no fever, but bacilli in sputum. By the rule I will lay down the ratio of movement of the affected as compared with the unaffected side, namely, 1 to 3, with so small a spirometrical record, makes him an unfavorable case for tuberculin treatment. His case is in arrest. Nature and high altitude climate have already accomplished what tuberculin might perhaps have done. They work together on somewhat the same lines. Of course reactions, both local and general, followed, up to the limit of 6 mg., which

was reached in his case. He, undoubtedly, had fibroid tissue enough and the course was not satisfactory. He has since been doing first rate on mercurial inunctions over the hardened lung, while camping out at about 7,000 feet elevation in the mountains.

*Case 16*, a young man, age 20, from Scotland, a sub-acute case, diagnosed fibro-tuberculous, second stage, right, and first left. I believe the tuberculin treatment could not compass this case, for the principal reason which operates in many other serious cases, lack of ventilation of the chiefly affected lung, and also, in this case, because of a concealed septic state. Though 14 mg. were very gradually reached in the course of six weeks with some improvement, yet some times even a greater afternoon rise of temperature would be shown, 102.3°, when the tuberculin had not been given than when it had. The abandonment of tuberculin and the substitution of iodine and chloride of gold and sodium injections for three weeks, greatly controlled the fever, and he is now doing fairly well.

*Case 17* I think was peculiar from the too great amount of elastic tissue in his expectoration, which followed a diminution in amount of sputum from 1 pint to 4 ounces per diem, during the first two weeks of tuberculin treatment. He was a blacksmith, age 35, from Omaha, Neb. His was diagnosed as a case of broncho-fibrosis, both lungs affected. Second stage right, and first, with large bronchiestasis left.

This case prompted the suggestion that it is possible excessively to stimulate the production of fibrous or elastic tissue, which is, of course, unadvisable in those already having enough. So although bacilli in decreasing numbers were still found in his sputa tuberculin was discontinued at 12 mg.

*Case 18*, age 30, Secretary of a Cattle Company in Canon City, originally from Kansas for his health. A large cavity on left lung with some slight fibroid involvement of the rest of the left and part of the right lung. There was slight oozing of blood into the cavity, under one of the reactions; suggesting caution in advancing the treatment; and 12 mg. in a month was the highest point reached. In his case, other than the cavity, there was decided local improvement,—an increase and then a decrease in the number of bacilli to the field, an effect of treatment which was shown in nearly all of these cases.

*Case 19.*—A young man aged 24, from Kansas. Disease originally started from left pleurisy with effusion. February 17, before tuberculin treatment, the diagnosis was, left third stage, fibro-tuberculosis and catarrhal phthisis and right first fibroid. During the next four weeks 17 mg., the largest dose, was reached. The patient increasing from 128¾ to 135 pounds in weight, and being very much encouraged by his ability to do and his general improvement. He then had an attack of what seemed to be la grippe, and no more injections were given. A week after this he was taken down with a very bad attack of measles. After ten days these accumulated poisons seemed to have centered in his left lung, which broke down rapidly, and he died one month after these tuberculin injections. I do not regret the course taken in this or any of the cases to whom tuberculin was administered, for accidents like that last mentioned could not be foretold, and the only disparaging claim that could be made would be that this young man was perhaps less liable to stand the peculiar poison of measles than if he had not been under the Koch treatment. Some of these patient are present for you to see them, and my charts and records of these reported cases are here for your attention if you desire it.

Case 20, much like No. 16, only worse as to febrile change and impossibility of thoroughly ventilating a contracted and partly honey-combed left lung. The pneumatic cabinet might have been of use in this case in counteracting the shrinkage effect of tuberculin in upper half of the affected left lung. This, as before explained, shuts in the stagnant air and perhaps the exuded bacilli in the lung below; and such a case is not a good one for pushing the treatment.

These are the only cases to whom I have administered tuberculin, except two cases this week commenced, and a small dose of ¼ mg., given to a forlorn case for diagnostic purposes only. It was desired to know if the only remaining lung (the left) was tuberculous, and this experiment by local reaction verified the suspicion that it was. The Shurly treatment was then pushed with some good effect, but the patient died a month or two later.

On the whole, considering the very serious nature of the lung conditions in these, all of them tuberculous cases, the result of the treatment is very encouraging, as proving tuberculin to be an important aid to climatic treatment in this region; or *vice versa* if you prefer it that way.

I ought perhaps to say that during this time, covered by my tuberculin experiments, I refused or decided not to give the Koch treatment to more than twice as many patients, partly because they were unsuited, but in a majority because they were incipient cases, especially suited to our climatic treatment.

We have been asked or have ourselves desired, to use this newly discovered agent—tuberculin—without any very distinct understanding being granted us as to its composition, its strength or its mode of preparation. In view of reported adverse experiences and statements in reference to its use, this enforced uncertainty as to detail naturally suggests the inquiry: would it not have been better to have submitted the first use of tu-

berculin, with all available information with reference thereto, to carefully selected experimenters for their separate determination of rules of procedure, and then from these sources reliable directions as to detail could have been sent out to the world. As it is, we now hear of Prof. Koch reducing his initial dose in certain cases to one-tenth of his first promulgated order.

The advisability of the method employed for putting the tuberculin into use, was questionable, that is, its only being sold to hospitals. Of course one could use the friendship of the Secretary of the Interior, as I did, and get some of the "desired stuff," as he called it when he sent it to me. Nevertheless, the arbitrary restrictions to only hospital use conjoined with the directions that patients should be put to bed when reacting the lymph, was, I believe, a mistake. It was suggestive of the use of too large doses in too serious cases, and a lack of a right conception of what was desired to be accomplished. I started out to follow the direction with a private hospital of ten beds, but soon found it unnecessary for the kind of patients who do best under this treatment. To be out of doors, riding on tramway or cable cars, even while gently reacting, was not hurtful, and my most satisfactory Koch patients were those who regularly exercised; as No. 1, who is a furniture polisher and has kept at his work ever since the first week of treatment; No. 2, who is all the time *on the go*; No. 5 ditto, whose expansion has increased to 4½ inches; and Nos. 8 and 9, who, though reacting to less than one mg., have been averaging five to eight miles walking daily.

As to the details of the necessary examination, preliminary to commencing treatment with tuberculin, I have not time to give all the particulars. You can see my own examination chart which I here present; but I know that ordinarily some of the most essential points are omitted by the average medical man. Take the manometer, spirometer, and mensuration, for instance. The manometer, which gives information in millimetres of mercury of the strength of new elastic tissue formation in the lungs, is very seldom used; the spirometer, which has been depreciated by some of our best authors, the late Dr. Flint, Sr., for instance, is not one-tenth appreciated by the medical profession; I do not know that the bilateral movements of the chest are generally taken, and the same can be said of proper stethoscopic percussion for determining the commencement of softening or excavation of lung tissue. As I have made a careful use of these extra aids, to a proper understanding of these tubercular cases, I may perhaps be excused for referring to some thoughts which seem to me essential.

PRECAUTIONARY MEASURES AND RULES:

Understand the patient, the extent and character of his disease and its abridgement of his respiratory capacity, or the liability to such abridgement under this treatment. For this purpose compare the chest expansion and spirometrical record with the normal for his height; and if, as is most likely, the two sides differ in movement, compare the bilateral movements with the spirometrical record of this given individual. For instance, as in case 13 of my list, the movement of the left as to the right side was as one to three, and the spirometrical record 150 cubic inches, when it ought to be 250 for a healthy man of his height. Well, if we were to calculate that the right lung does its full share it would leave only 125 cubic inches for the left, or on the basis of one to three of what was breathed, 37 cubic inches should be allotted to the left lung, and this only on forced breathing to the extreme. Of course this left lung was not ventilated. If there has ever been any softening in such a case and it has reached such a stationary stage, as the fever curve, that tuberculin would be otherwise safe to administer, then it must be that nature has been already at work through her own method of cure—the fibroid process. She has probably pasted up the lung by her plastic exudations of fibroid deposits, even to adhesions of that lung to the chest wall, so that the bacilli, though numbered by the million, lie embedded in natural prison vaults, almost effectually shut off from again invading the rest of the system.

Among the records of arrested phthisis in this rarefied atmosphere, we Colorado physicians have all seen this natural arrest. In fact, this fibroid process is so evidently Nature's chief reliance in all successful, as well as all fatal cases, that we are led to ask—how can arrest after softening of lung tissue occur without the aid of this natural method? Why, then, since the character of ausculation sounds, and the method of healing caused by mild reactions to tuberculin are so similar to the effects of the fibroid process, only more accurate, why, I ask, should we go outside of natural causes to explain the curative process of Koch's lymph? It is entirely unnecessary. The lack of understanding of this matter is so queerly shown even by men of high repute.

Dr. Bristowe, who opened the late discussion of Koch's method in the Metropolitan Counties Branch of the British Medical Association, scouted at the idea that the bacilli were not killed but simply imprisoned "in an excess of inflammatory products," and Dr. Hector Mackenzie, equally antagonistic in his remarks, claimed that "Koch had led us to suppose that he had himself fully tested this wonderful remedy of his, that he had found that it prevented the growth of the bacilli in the test tube," etc. Dr. S. K. Jackson, of Norfolk, Va., in his late article on Tuberculin, read before the Section of Practice at the May meeting of the A. M. A., in Washing-

ton, entered into a lengthy argument to show that the bacilli *in situ* are killed by the introduced ptomaines of artificially cultivated bacilli, in accordance with a law which he had some time ago and several times since announced that "no organism can live in its own excreta." The test tube has nothing to do with a natural process like that we are discussing, and as for "poisoning" the bacilli in possession of the battle ground in the lungs, by their ptomaines introduced into the blood hypodermically, I am myself inclined to believe it would be disastrous to the human body, or to any other body, to do such a thing. It does not seem reasonable to me that enough of the ptomaines diluted by the blood would reach the bacilli to poison them. Koch himself says that they are not destroyed. It is now perfectly clear to my mind that *that* is what does occur where Koch speaks of the "imprisonment of the bacilli" in inflammatory tissue, provided that *inflammation* is the proper term to use. But I am led to believe that Koch made a great mistake when he put it before the world that it was the "necrosis" (death) of the affected tissue which was effected, or ever desired to be effected. No, to kill the part, in order to destroy the enemy contained therein, would be such an aggravation, or rather perversion of the splendid healing process at the bottom of his discovery, that no one could gain an advantage from so doing, unless it would be Prof. Virchow, with his twenty-six post-mortems on bodies defunct after tuberculin treatment. The renewal and healing of given tissue affected is not to be brought about by the death (necrosis) of that tissue, which means its extinction. It is not necrosis that takes place or is desired, but a stenosis, through the substitution of new material or the quickened action of Nature's fibroid process, in answer to a local demand. If it were not for an unusual cause, which is the tuberculin in the blood, nothing out of the ordinary would happen. But that which happens is unusual. Here I am led to a confession of faith, which is sufficiently strong to support me in the further use of tuberculin in my practice, though every other physician in America should pronounce against it.

It is a beautiful idea, abundantly verified as the law of Nature in her warfare with disease, that every living tissue is endowed with a certain power of protection against injury, and repair for the same. This power is inherent in, and manifested in harmony with, the activity of the *life principle*. So an acute injury of the delicate lung tissue, where the circulation of blood is five times as rapid as in the capillaries of the service of the body, must result in a correspondingly greater systemic disturbance, and local demand for repair. Therefore, when an unused, poorly ventilated, catarrhally affected portion of lung becomes the congenial and fruitful abode of man's greatest enemy—the bacilli of tubercle—I believe the call is not unheeded by this independent thought of nature. The resources of the whole system are called upon to aid in the burning out of the enemy by combustion, drowning or washing him out in the rivers of catarrh, or burying him beneath or behind the ever-encroaching fibroid battlements. Where else will we find this protective influence more acute than at that point, especially in the lung, where the bacilli are lodged, and already surrounded by *these sensitive, protective elements?* It is not unreasonable to believe that the fibroid tissue which surrounds tubercular spots or ulcerations, and whose chief function and tendency is contraction, should be unusually susceptible to the ptomaines or juice of the bacilli, were these poisons to find their way there through the general circulation. I believe that this is the case, for I have several times verified the contraction of fibroid elements in affected lung tissue under the special stimulation of tuberculin.

I know I shall be deemed delinquent in this article if I do not mention the action of the Philadelphia Commission and the Johns Hopkins College Hospital, in discontinuing the use of tuberculin because they were disappointed with their results.

As to the Philadelphia physicians, whom I highly esteem, I wish to say this: They were scrupulously following Prof. Koch's directions as to *dose* and *rules of confinement*. I am not. The character of the cases chosen at both the above trials were serious enough, notwithstanding a lurking suspicion in my own mind that had they properly tried stethoscopic percussion in some of their cases, "with tubular breathing and moist crackling râles at an apex," they would have detected excavations *before the treatment* instead of referring an advance in disease to the tuberculin injections. Of course, the contraction of affected tissues around a softened spot makes a small pre-existing cavity more evident.

The Johns Hopkins College Hospital failure is not to be wondered at. They could not surely have expected to have a miracle performed; for had they started out to damn the Koch treatment they could not have done so more effectually than by selecting such a bad lot of cases, housing them in an hospital, and giving them such large doses of tuberculin. Look at the list of the first eleven cases reported. Five of them with both lungs considerably affected; seven with large cavities, and the others with softenings commenced or going on, *i. e.*, according to the physical signs noted in the first report. Had these cases been set loose in the country during the time of the Koch treatment, and reached the maximum of 4 mg. instead of 12 to 20, they would probably have had enough for their needs. I cannot see that the essential precautions men-

tioned in this paper have been taken in any of the experiments thus far reported in the journals. The nearest approach to their observance, which have furnished equally favorable results to my own, have been by Dr. Karl von Ruck, of Asheville, N. C. I was very glad to see from his late paper in the *Medical Record* that he had been working on much the same line as I had. I do not notice in his reports the extravagant general and febrile reactions so grossly exaggerated in newspaper reports of certain so-called "Koch hospitals."

CONCLUSIONS.

The following conclusions, however poorly elaborated in the body of this report, seem to me to be warranted at this stage of our investigations.

1. Tuberculin furnishes a natural method of cure, to be classed with stimulants, hypophosphites, exercise, high altitudes and cold, dry air; yet it is too powerful to be indiscriminately used, and the proportion of consumptives for whom its use would be advantageous remains to be determined.

2. It is unreasonable to expect that tuberculin will be useful in sepsis, or any poisonous blood state other than tuberculosis.

3. Use tuberculin not as a substitute, but as an aid to climate, and other natural methods of cure—proper food, hygienic surroundings, mountain climbing, etc. Especially are high altitudes and exercise indicated, if otherwise advisable, because they favor the ventilation of lung tissue, which is necessary in giving tuberculin.

4. Seek only local reactions in the affected tissues. A high or prolonged febrile reaction is to be avoided, if possible. Use the minimum dose that will accomplish this purpose, and it is preferable to let at least a day elapse between a subsidence of one reaction and the possible creation of another. Remember that an increase from ¼ to ⅓ of a mg. may be as great for a case already reacting to the smaller dose as an increase from 30 to 40 mg. in a case that is far advanced in the treatment.

5. The lack of ventilation in a lung, whether caused by tuberculin already used or pre-existing, is a decided contra-indication to pushing the treatment, *i. e.*, according to this deficiency and the febrile state.

6. The test for the proper ventilation of a tubercular or fibro-tubercular lung is to be found in the comparison of the spirometrical record with the bilateral measurements of the chest and with available physical signs. A fair way to express deficient ventilation is to say, if the normal spirometrical record for the height is abridged one-half, and the movements of the two sides are as one to two, then there is sufficient lack of ventilation in the weaker lung for exercising extra caution as to using tuberculin at all. There is either too much elastic tissue already there, or the bacilli, if loosened by tuberculin, may not be expelled.

7. The stage of the lung disease has not so much to do with the question of using tuberculin at all, as has its extent and locality, the febrile course and the ventilation of the affected lung. If possible, have an average daily range of temperature of less than two degrees F. upon which to proceed, and the patient able to be much of the time in the open air.

8. The diagnostic value of small hypodermics of tuberculin is shown with or without febrile reaction in the appearance on auscultation at suspected or affected spots in the lung substance, of a more or less decided *roughening of a previously existing broncho-vesicular respiration;* or a *high pitched, rude breathing* where before only a feeble respiration was noticed.

9. With the appearance of any unfavorable physical signs during the course of tuberculin treatment, such as a clogged up section of lung, or scattered moist râles suspicious of softening, I think it would be well to intermit injections of tuberculin and substitute those of iodine for a while, or to give temporarily syrup hydro-iodic acid compound, or to use inunctions of oliate of mercury over the affected lung, until a return to the use of tuberculin is quite safe.

10. As to permanence of effects produced by tuberculin injections, there is to me no apparent reason for expecting any more lasting results than from any other agency which will accomplish as much. I do not see any reason why bacilli should not appear again in the sputum after their cessation under tuberculin treatment, as after their cessation under climatic change or the double chloride of gold injections. It is quite probable that the effect of tuberculin is temporary. We need not look for a guaranteed immunity from lung tuberculosis except under such other conditions of healthful living as would insure it.

11. In fine, tuberculin is not a remedy to be chosen for the great majority of pulmonary consumptives in preference to a proper change of life and climate, for it does not equal the latter; but with such a change, it is an important adjuvant to the cure of a large selected class of tubercular lung diseases.

Denver, Col., June 15, 1891.

BEQUESTS TO MEDICAL CHARITIES.—By the will of the late Mr. John T. Farish, who died recently in New York, charitable bequests were make to St. Luke's Hospital, $50,000; Home for Incurables, $50,000; New York Cancer Hospital, $25,000; Hospital for Ruptured and Crippled, Nursery, and Child's Hospital, House of Rest for Consumptives, and Manhattan Eye and Ear Hospital, each $10,000.

## LOCATING THE BALL IN A CASE OF GUN-SHOT WOUND IN THE CERVICAL REGION.

*Read before the Chicago Pathological Society, June 8, 1891.*

BY J. J. M. ANGEAR, B.Sc., M.A., M.D.,

PROFESSOR OF PHYSIOLOGY, NERVOUS AND MENTAL DISEASES IN THE COLLEGE OF PHYSICIANS AND SURGEONS, KEOKUK, IOWA, AND PROFESSOR OF PHYSIOLOGY AND PATHOLOGY IN THE U. S. DENTAL COLLEGE, CHICAGO, ILL.

Twenty-second day of August, 1889, I was summoned to see Mr. W., in the south-east part of Kansas, under the care of Dr. D. W. King, of Wier City, Kansas, whose history I learned as follows: Mr. W. was born in Henry County, Ill.; was 33 years old; had lived in Cherokee County, Kansas, nineteen years; married, had no children.

On the 11th day of May, with others, he was out hunting, and was accidentally shot in the neck by a glancing shot, with a 22 caliber rifle ball. The manner in which it occurred will be better understood by referring to the diagram.

The tree, C, was in a field. The line A B represents a hedge which divides the field from the road. E and F represent two men who fired at a bird on a branch of the tree, C. A ball from one of the two men's rifles E or F, struck the branch on which the bird was, and glanced and struck Mr. W., at D, a distance of 105 feet, who at the time was intently watching for game in the hedge, carrying his gun in his hands, cocked. His body was leaning forward, head still further forward, and thrown toward the right. The ball struck him a little back and inward of the point of the shoulder. He fell on his face across his gun. He experienced no pain, but could not breathe. He supposed that his gun had gone off and the rebound had struck him in his stomach, and had "knocked his breath out." It was some time before he could breathe; there was no hæmorrhage.

Dr. King could not follow the direction of the ball with the probe, because the probe could be more readily passed between the muscles. And it was impossible to put the body in the exact position that it was in when shot.

His temperature rose to 105½°, pulse 110 and remained so for one week, when temperature fell to 103½°, and the pulse to 100. The temperature remained at 103½° for eight weeks, when it fell to normal and remained so for two days only. For the first four weeks he experienced excruciating pain in his shoulders. Dr. J. D. Griffith, of Kansas City, was called May 19, 1889. He operated, with the view of removing the ball, by cutting down upon the spinal column, and found the lammæ of the sixth cervical vertebra badly fractured and slightly pressing on the cord, but no hole in the lamina. He removed the spinous process of the sixth cervical vertebra with the bone forceps, and with a large trephine removed the lammæ and picked out the fragments of bone. The dura mater was badly torn; he removed the torn portions with the scissors. The cord was not wounded nor contused. He did not find the ball so he closed the wound, which healed by granulation, readily. The next day severe nervous rigors set in, and spasmodic contractions of the muscles, akin to rigor mortis (tonic spasm). They continued for two weeks, in fact have never left him entirely. Drs. Agnew, Wyeth, Gregory and myself had been consulted by letter. Prognosis given was unfavorable.

I saw him August 23, 1889, three months and twelve days after the accident, and found him as follows: Mind as clear as ever, paralyzed below the neck on both sides, except the deltoids. He could lift his arms and move his little fingers slightly. His face was dark with fine red spots or points, such as we sometimes see in cases of epilepsy. Breathes well while lying down, but with difficulty when sitting up. The muscles of the back of his neck are paralyzed, and his head drops forward upon his breast; when his head is brought forward far enough so that the centre of gravity of his head falls in front of its support then he stops breathing, becomes entirely unconscious; by lifting his head and putting him on his back, so that his neck is straight, in a short time he breathes, becomes conscious. He looked around in a bewildered manner and asked, What is the matter? Where have I been? How long was it?

With ease and without pain he is able to move his head in every direction, except backward. No difficulty in moving his tongue; voice and eyes are normal, so are all of his special senses except feeling. At first he had to be cautious in swallowing else the material which he attempted to swallow would pass into the larynx. At present there is a great improvement in this regard. May say that deglutition is normal.

Rate of respiration, at first ran down in proportion, as the pulse ran up. This has improved, but the rate of respiration at present is not quite in due proportion to the pulse-rate. The pulse runs down to 40 per minute when the bladder is washed out.

The urine had to be drawn off every six hours, and would draw off from a quart to one-half gallon each time for the first eight weeks. Since

that time it has passed off of its own accord, but not unconsciously. There is a desire to urinate. He has but little control over the bladder. He can hold the urine for a very limited time, a few seconds only.

At first the bowels moved without cathartics, but not at present. He has a little better control over the rectum than over the bladder. The inferior extremities feel as if asleep, but better than they were. Not much of formication. Sensation exists all over the body, but very low, not able to recognize two prints of the æsthesiometer anywhere below the point of injury; but he can tell when and where he is touched or pricked, and says he could touch any part of his body he may be asked to, were he able to move his hands and arms.

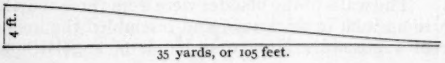
When the æsthesiometer is put across the limb, instead of recognizing the two points, he says it seems to him as if something were pressing upon or across the limb.

The heart and lungs are normal. Reflexes of the superior flexor muscles are present, but not the extensors. Did not test the knee-jerk. Could not get any ankle clonus. Tickling of the soles caused no contractions, but he said he felt it. No pain in his head except soon after the operation, and then in the occiput.

There is a sudden contraction of all the flexors of the body from the slightest irritation, as the movement of the bed clothes, or a draft of air strikes him. He will draw up his feet and legs, draw his hands up under his chin, his head forward, and bend his trunk forward; thus the body gets into a ball, as nearly as possible. No apparent atrophy of any of the muscles. Priapism just making its appearance very slightly. There is a large bed sore over the sacrum, which was started by leaving the bed-pan under him too long at one time.

The first thing is to locate the ball, if possible, then to determine a course of treatment, if anything can be done.

The branch of the tree that was struck instead of the bird, and from which the ball glanced, is about eight feet from the ground. The ground where Mr. W. was standing when shot is about eighteen inches below the foot of the tree. Hence the total elevation from which the ball came is nine and one-half feet. Mr. W. is five feet seven and one-half inches in heighth, and was stooping, and was shot a little above the point of his shoulder, hence the ball struck him about five feet above the ground. This height, subtracted from the total elevation, is about four feet six inches. Therefore the ball, in reaching its victim, had a descent of about four and one-half feet in 105 feet, thus represented.




35 yards, or 105 feet.

Practically the elevation was nothing in comparison to the distance—a little over one-half inch (.514) in a foot.

The inclination of the body forward and the head forward and to the right, more than counterbalanced the elevation from which the ball came. Practically it was as if he were standing erect and the ball came from below and from his left and behind him, hence the ball travelled upward and forward, striking the left lamina. This surface looks backward, downward and outward, which deflected the ball a little backward and upward. The ball must be above the point where it struck the spinal column. If the ball came with sufficient force to penetrate this lamina, it is to be found above the point of entrance into the canal. The distance it will travel upward in the spinal canal will depend upon the momentum that the ball had at the time of its entrance into the canal. This we have no data by which to make any calculation.

Our mathematics, or mechanics, tell us the ball is up above the point of entrance. There was but one wound, hence we know that the ball is in the neck. Either in the spinal canal or it had fractured the lamina and lodged just outside the spinal column. If this is to be told it must be told by symptoms and not by mechanics.

The nervous center for respiration extends from the upper part of the medulla oblongata down to the third or fourth pair of cervical nerves. There being no difficulty in breathing when the head is in a normal position, hence that part of the cord was not damaged.

The spinal accessory arises as low down as the fourth pair of cervical nerves; was not interfered with in its function, because voice was normal and he was able to rotate his head.

The brachial plexes arise a part above and a part below the fifth cervical vertebra. The circumflex animates, the deltoid and the ulner goes to the little finger; both of these were less affected than any other part of the brachial plexus. He could with difficulty lift his arm and slightly move his little finger. These all tell us that the ball is as high and no higher than the fifth cervical vertebra.

We must remember that the dura mater of the cord is not attached to the spinal canal, but there is a space between the cord and the dura, as well as between the walls of the canal and the dura, therefore we are able to introduce a probe between the cord and the dura or between the dura and the walls of the canal for two or three inches with impunity; for this reason we wondered why in the operation the ball was not found, and we were told that the lamina was fractured; but there was no hole through which the ball entered the canal. We began to doubt seriously that the ball was in the spinal canal. At this vital point, symptomatology is too cloudy to aid us to

dispel the doubts. If there are any symptoms or evidence of cut, lacerated or compressed cord, they were so completely covered by the symptoms of general meningitis and myelitis that they could not be recognized.

We gave our attention to the external parts of the neck in the vicinity of the fifth vertebra. We found a very tender spot opposite the transverse process of the fifth vertebra on the left side. If this was the ball, it was playing no part in the present nervous disturbance, and not being positive that it was the ball, he refused to have an exploratory incision made, saying he had been cut enough to no purpose. We soon came to the conclusion that the meningitis and myelitis had almost destroyed the cord. Had the ball destroyed the cord there would have been priapism from the beginning; but as it is, it is but just beginning, and it is to be taken as evidence that secondary degeneration has taken place, and regeneration of nerve substance under such circumstances, is not known to occur. Muscular atrophy follows destruction of the cord and there is no evidence of atrophy at present. Spasmodic paralysis is evidence of secondary degeneration.

The high temperature which existed for the first eight weeks is evidence of meningitis, and not injury of the substance of the cord. The contractions of the flexor muscles came from the myelitis. Thus far the degeneration is of the descending variety alone. Had there been any of the ascending variety, he would have died of asphyxia before this.

We have decided that the ball is up as high as the fifth vertebra. If we operate we must remove the transverse process and laminæ of the fifth vertebra. The process prominens, the spinous process and laminæ of the sixth vertebra are gone, and now to remove the transverse process and laminæ of the fifth vertebra, is to remove a large portion of the support to the head, and would be, at least, a mutilation which demands serious consideration, and not to be perpetrated without strong hope of bringing about such relief as to undoubtedly compensate for the great loss.

Should we cut down, find the ball and remove it, it would not remove the damage already done to the membranes and cord, nor would it put a stop to the chronic meningitis and myelitis, nor would it arrest the degeneration that is going on. It would aggravate all these pathological conditions, and simply hasten the final end.

This array of facts compelled us to abandon all thought of operating. We had an apparatus made to hold his head in position, with the hope that it might enable him to sit up, and even to be taken out of doors, and thus maintain his general health, give the bed-sore an opportunity to heal, aid the bladder in getting rid of the urine, which is becoming ammoniacal, and thereby lessen his suffering and prolong his days. With it he was able to sit up five or six hours at a time, but could not wear it all the time, it made him "so tired."

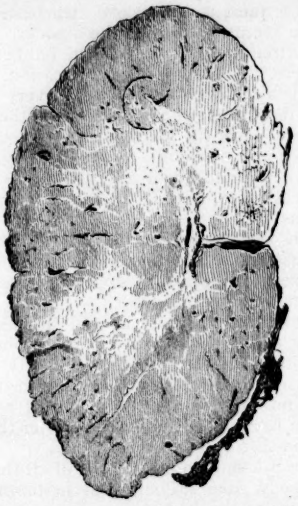


Figure 1.

Dr. King wrote me in substance as follows: "The cystitis became very bad, though the bladder was well attended to, was washed with various antiseptic washes; the urine became so offensive that it was very sickening to wash out the bladder. The priapism became very troublesome, also the tonic spasms of all the flexors. The muscular atrophy was very great before he died."

He died September 5, 1890, one year, three months and twenty-four days after the accident. Ten days previous to his death he had been passing from his bladder a material resembling prune juice in color and consistency. Pulse full and strong, 120 per minute. Temperature 103°, respiration 20, with sterterous breathing.

Post-mortem three hours after death. A deposit had been thrown out over the cord where the lamina had been removed. The ball struck the lamina of the sixth cervical vertebra and deflected upwards, and penetrated the dura mater on the right side of the cord, at a point corresponding to the middle of the fifth cervical vertebra, where it now remains. The fourth, fifth and sixth pair of cervical nerves, for a distance of an inch from the cord, showed traces of atrophy, but the brachial plexus was normal.

The walls of the bladder were from three-fourths to an inch in thickness, and resembled the inside of a gizzard. Numerous bands of a gelatinous

substance, black and gritty, formed an artistic network over its surface. It was extremely attractive to the eye, but disagreeable to the nose.

Figure

The same prune juice-like material was in the ureters and kidneys. Numerous calculi were

Figure 3.

found in them. The kidneys were enormously hypertrophied, and especially the right one. The liver was enlarged. The stomach, heart and lungs were normal. It is very evident that death was caused by uræmia. The cystitis was caused from the injury from the cord. Nephritis was set up by the inflammation of the bladder. The nephritis caused the uræmia.

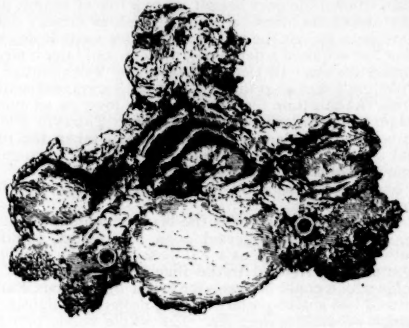


Figure 4.

Microscopic examination of the cord just below the ball: No. 1, shows the cord as a whole. All normal appearances of the cord are destroyed by the myelitis.

No. 2, a lateral portion more highly magnified, showing the hypertrophied neuroglia and blood-vessels, but it fails to bring out any motor cells or nerve tubules.

No. 3 shows the thickening of the membranes, the result of the meningitis.

Fig. 4. Fifth cervical vertebra, showing the ball between the cord and membranes.

---

### A Case of Hermaphrodism.

Prof. Polailon has recently[1] reported an interesting case of hermaphrodism. The patient, Marie P., æt, 25, was referred to him July 1887, for the purpose of having an operation made, because of absence of the vagina. The young woman had never menstruated, nor had she had any periodical sickness, nor congestion, nor hæmorrhage of an organ taking the place of menstruation.

The external genital organs were well formed. The mons veneris, the labia majora, and labia minora were normal. The clitoris was of ordinary dimensions and appearance. Below it the meatus urinarius occupied its usual place. But behind the fossa navicularis the vagina was represented by a depression scarcely two centimeters in depth. Above this depression the most careful examination failed to reveal the slightest trace of a uterus. At the level of the external inguinal ring on each side there was a swelling resembling a bubonocele. Each swelling contained a hard, ovoid, mobile body giving a peculiar sensation on pressure. On coughing there was only a slight propulsion of the intestine behind or beside these organs. She had never observed that these organs became more voluminous or more sensitive at certain times. In physique she had all the characteristics of femininity. In stature a little above the average, her pel-

[1] Soc. Obst. et Gyn. May 14, 1891.

vis was large, and hips prominent. Her breasts were well developed. Her skin was soft and fine, and a moderately thick panniculus adiposus spread over the trunk, and rounded out the limbs. There was no trace of beard on the upper lip or chin; the larynx was not prominent and the voice had the feminine timbre. Finally, her feelings and tastes were those of a woman. As the result of the examination an operation was declined. Not disheartened she gave herself up to a life of shame, and her lovers soon formed a vagina as well as surgery could have done it. At the end of a year the vaginal depression had acquired a depth of 5 to 6 cm. and later it measured 7 to 8 cm. In October, 1890, she was admitted to Polaillon's service at the Pitié sick with a grave albuminuria. At this time the vagina was as long as an index finger and admitted a cusco speculum. The skin folded in between the bladder and rectum had taken the rosy tint and softness of a mucous membrane. The two small tumors had not changed any.

She died December 17, and the autopsy gave the following results:

The peritoneum covered the bladder, which was normal, and was then reflected upon the rectum, forming between the two organs a cul-de-sac. On the sides the peritoneum extended to the iliac fossæ, and formed no folds which could be taken for broad ligaments. But it formed two antero-posterior folds, projecting slightly, to which we shall again refer. The whole pelvic peritoneum was lined with a layer of smooth muscular fibres, disposed in interlacing bundles. On dissecting off the peritoneum there was no trace of ovaries or tubes found beneath it. Nor was there a uterus. But in the place where the uterus should have been found, below the recto-vesical cul-de-sac, there was a thickening of the tissues, forming a lump about the size of a bean. This lump was situated in the median line, a little above and in front of the bottom of the artificial vagina. It was so small that it was impossible to detect by vaginal touch. From each side of the lump in the median line, a cord passed obliquely forward, outwardly and downwardly, ending in the bodies contained in the inguinal hernias. These two cords were each composed of a conduit resembling a *vas diferens*, and of vessels encircling it. The inguinal tumors each contained an ovoid organ, white in color, with a smooth surface, and enclosed in a serous cavity analogous to a *tunica vaginalis*. The form and disposition of this organ differed slightly on the two sides.

On the left the large end of the ovoid was situated high in the inguinal canal; the small end was disposed in the form of a crinkled body which adhered to the inguinal ring, and which received the left cord. The serous tunic surrounded only the upper end of the organ, extending into the inguinal canal, but without communicating with the peritoneum.

On the right the organ was more rounded, shorter, more regular, and better projected from the inguinal canal. It had clearly the aspect of a testicle. It was surrounded by a vaginal tunic which adhered to its vaginal portion as far as the level of an epididymus thickening from which the right cord came out.

On each side a vasculaire bundle, which represented the spermatic artery and its veins, descended below the peritoneum to supply the inguinal organs, which were evidently testicles.

On a transverse section of the right gland there was seen, at the periphery, the *tunica albuginea*, and at the center, characteristic testicular substance. In a longitudinal section of the left gland there was likewise found the *albuginea* at the periphery, testicular substance in the center, and at the lower portion of the gland, the body of Hygmore, a rudimentary epididymis.

The histological examination, made in M. Cornil's laboratory, showed in the testicular substance seminiferous tubes, with thick, sclerosed walls, filled with atrophied epithelial cells. They were then two male organs, but atrophied, sclerosed organs, and without physiological activity.

The attention of the histologists was also directed to the small retro-vesical lump, whence emerged the deferent canals. The question to be settled was whether it was a rudimentary uterus or a vestige of prostate. A very minute examination revealed only muscular fibers extending in various directions, but no cavity lined with epithelium, and no glandular formation. It was therefore neither a uterus nor a prostate, but simply a thickening of sub-peritoneal muscular tissue.

The neck of the bladder was then searched for the vestige of a prostate. At this point there was found a rounded organ which embraced the origin urethra, and resembled a small prostate. Microscopic sections showed that this organ was formed of muscular bundles, the interstices of which contained glandular formations. It was, then, a rudimentary prostate, but a prostate without prostatic utricle, and without ejaculatory canals. The *vesiculæ seminales*, moreover, were wanting entirely. On each side of the vulvar orifice were two very distinct vulvo-vaginal glands, with their ducts opening at the vulva.

On continuing the autopsy, there was found an interstitial nephritis of both kidneys, a congested and inflamed spleen, a well developed pancreas, and a liver of normal appearance. There were serous effusions in the pleural cavities and pericardium, and the heart presented a slight degree of endocarditis. A most striking feature of the autopsy was the small volume of the arch of the aorta, which could not admit the little finger into its cavity.

The genital anomaly classes this individual among the false hermaphrodites of the androgynous variety. In fact, Marie P. was a man. But the presence and very advanced evolution of the testicles had not impressed on the organism the characters of the male sex, or only some of these characters. On the other hand, a consideration only of the appearances and the external forms indicated a woman affected with an absence or imperforation of the vagina.

From the surgical standpoint, there was no indication for the creation of an artificial vagina, for there was no uterus, nor upper cavity, forming a vaginal vestige, toward which a route might be cut. Under these conditions it is the rule to withhold operation. In fact, in opening up the recto-vesical space to a certain extent, there is obtained only a limited conduit, with bleeding surfaces, which continually tend to reunite by cicatrization. The result obtained can not be kept because of the cicatricial retraction. But the observation of this patient shows that in similar cases a vaginal conduit can be slowly formed by pushing in the skin between the bladder and rectum with a large mandrel. In this way the bladder could be separated from the rectum, the vulvar mucous membrane pushed into a cul-de-sac, and a sort of vagina created, which could be subsequently completed and maintained by sexual approaches—*Journal de Médicine de Paris.*

---

THE COMBINATION of cocaine and antipyrin is said to produce a more lasting and complete anæsthesia than cocaine used alone. Dr. Martin (*Med. News*) suggests, for powerful local anæsthetic effect on the gum and sensitive dentine, the following solution:

Cocaine Hydrochlorate crystals, ¾ gr.
Antipyrin, 6 grs.
Water, 16 drops.

Our own observation inclines us to prefer 2 grains acetanilid to the antipyrin.—*New Remedies.*

---

BERLIN gives the carriages of physicians the right of way through the crowded streets. The coachmen wear a distinctive white hat.

THE
Journal of the American Medical Association
PUBLISHED WEEKLY.

SUBSCRIPTION PRICE, INCLUDING POSTAGE.
PER ANNUM, IN ADVANCE . . . . . $5.00
SINGLE COPIES . . . . . 10 CENTS.

Subscriptions may begin at any time and be sent to
JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION,
No. 68 WABASH AVE.,
CHICAGO, ILLINOIS.

All members of the Association should send their Annual *Dues* to the *Treasurer*, Richard J. Dunglison, M.D., Lock Box 1274, Philadelphia, Pa.

BY-LAW IV.—THE PUBLICATION OF PAPERS AND REPORTS.

No report or other paper shall be entitled to publication in the volume for the year in which it shall be presented to the Association, unless it be placed in the hands of the Board of Trustees on or before the first day of July. It must also be so prepared as to require no material alteration or addition at the hands of its author.

Authors of papers are required to return their proofs within two weeks after their reception; otherwise they will be passed over and omitted from the volume.

Every paper received by this Association and ordered to be published, and all plates or other means of illustration, shall be considered the exclusive property of the Association, and shall be published and sold for the exclusive benefit of the Association.

The Board of Trustees shall have full discretionary power to omit from the published *Transactions*, in part or in whole, any paper that may be referred to it by the Association, or either of the Sections, unless specially instructed to the contrary by vote of the Association.

LONDON OFFICE, 57 AND 59 LUDGATE HILL.

SATURDAY, JULY 18, 1891.

## THE BACTERIOLOGY OF THE UTERINE CAVITY IN ENDOMETRITIS.

Since the observations of Noeggerath, increased attention has been devoted to infections of the genital tract of women. Whilst it must be confessed that the views of Noeggerath present many striking facts in their support, yet it cannot be denied that too much stress is laid upon one inficiens, namely: the gonococcus, and too little upon other pathogenic forms, that possibly play a most important rôle in these inflammations. Our attention is again called to this subject by Alexander Brandt, of Slarjansky's Clinic, St. Petersburg, (*Centralblatt für Gynäkologie*, June 20, 1891), in an important preliminary communication.

It consists of a careful bacteriological examination of the uterine cavity in twenty-five cases of endometritis, presenting the following varieties: E. hæmorrhagica, eleven; E. catarrhalés chronica, nine; E. gonorrhoica, four, and E. septica, one. Apparently every effort was made to obtain the contents of the uterine cavity free from admixture with the secretions of the cervical canal, the latter being thoroughly cleansed before going beyond the internal os. The uterine cavity was then scraped with a Volkmann spoon and the removed portions examined microscopically, and cultivations were made upon solid media. By cultivation, twenty-two of the twenty-five cases showed the presence of microörganisms. In two cases the streptococcus pyogenes were identified. A pure culture of this germ was obtained from the case of septic endometritis; in two cases of hæmorrhogic and one case of catarrhal endometritis, the staphylococcus pyogenes aureus were found; in two cases, alone, and in one, associated, with the staphylococcus pyogenes albus. This latter germ was found, alone, in two cases of the hæmorrhogic form.

Pure cultures of these germs were made according to Koch's method, the results showing that in seven cases, or 31 per cent., pathogenic microörganisms were found. Inoculation experiments were performed with the pure cultures, demonstrating that in each instance the streptococci and staphylococci retained their full virulence.

It will be readily admitted that the mere association of a lesion and the presence of a germ, does not indicate a pathological nexus; indeed if the latter is admitted, it does not follow that the disease is the product of the germ. Still, if we put these questions aside, for the moment, and come to the treatment, we will find but few surgeons, who do not believe in the prompt and thorough removal of the suppurative bacteria. When present upon a diseased membrane, whether primary or secondary in their operation, they can but do harm. Such being the case, we conclude that the observations of Brandt, in accord as they are with older researches, confirm the value of antisepsis in the treatment of the uterine mucosa, as indeed of all other mucous membranes.

## EARLY PREPARATION FOR THE SECTION WORK.

The true foundation of the scientific work of the American Medical Association lies in the Section. It is there that the real work is done, and the chief interest of members is found. The general sessions may profitably discuss the broad questions of medical education, ethics and legislation which alike affect all, and in which all are interested.

To our mind a continued growth, and increase

in the usefulness of the Association, largely depends upon a strengthening of the work done in the Sections. This, if accomplished at all, must be largely through the efforts and activity of the Section officers. There now remain eleven months before the next annual meeting at Detroit; it is none too soon to begin preparation for that meeting. We feel that we cannot urge this point too strongly, as we know that the Section work is commonly left to the two or three months preceding the date of meeting. This is all wrong, as it leaves too little time for the preparation of papers and discussions. The result is that the programme is often hurriedly made up and badly arranged, to the serious detriment of scientific work.

It is to be hoped that work will begin now for the next meeting. Section officers should begin correspondence with contributors, discussions should be arranged, and so far as possible the work mapped out. It will be found that there is none too much time for the preparation of a clean cut, well arranged programme.

We would especially call attention to the value of set topics for certain sessions, upon which one or two papers are prepared, and then several members are designated to take part in the discussion. This plan has worked well in many foreign societies, and has to some extent been adopted in our Association. Nothing is more attractive to a writer than to know that his paper will be intelligently discussed, and nothing contributes more to that end than a well arranged programme and precision in the work of the Section.

An effort will be made at the Detroit meeting to secure verbatim reports of the discussions.

## NATURE'S FOOD.

Nature's food for the infant is the milk of the mother. That it is the best food for the infant is accepted by all. But this means that it is the best food for the healthy infant. Nature has not provided a food for the sick child, particularly not for the child suffering from bowel disorders. Statistics abundantly prove that a large majority of the infants dying from the bowel disorders of summer are to be found among those who have been deprived more or less completely of their natural nourishment. From this undisputed fact it has been too often inferred that the proper food for the child affected with summer complaint is mother's milk, or in its absence that substitute which may be supposed most closely to resemble it. This is clearly a *non sequitur*. The conclusion should have been that the natural method of feeding the infant is a most excellent prophylactic against bowel trouble.

But to go farther, and to assume that a prophylactic measure will be a curative measure is clearly unwarranted. Vaccination will not cure variola.

The various efforts which have been made and are being made to imitate mother's milk, in its physical, chemical, and biological aspect, are worthy of the highest respect, but even should they reach an ideal conclusion, it will be to supply a food for the healthy baby, and not for the sick one. When sterilized milk was offered to the profession its advent was hailed by many as an important step forward in the feeding of sick babies, but its shortcomings are now becoming apparent, and are attracting the attention of medical writers.

The various abnormal fermentations which occur in the intestinal tract of the infant suffering with bowel trouble and which can be maintained by a milk preparation of one kind, can be maintained by a milk preparation of another kind. A microörganism which can grow in one milk food, can grow in any other milk food. To sterilize milk, and then introduce it into a nest of bacteria is a trifle illogical. And this illogical procedure has had its origin in the effort to imitate Nature's food.

If now, instead of stating with an hypothesis, the ground had been cleared of fetiches, and attention directed to the real problem, no doubt a much more tenable position would now be held on the subject of the proper food for a diarrhœal baby. The starting point for observation is a simple one: Abandon all hypotheses, study the conditions present in the child, and adapt the food to those conditions, on principles which shall be comprehensive.

## IMPURE ICE.

The heated term is upon us, and with the season comes the crave for cooling drinks. The carbonated drinks furnished by the shops are

usually healthful, but too frequently there is a temptation to indulge freely in the water of melted ice. This, when gathered from impure sources, is just as unfit for drinking and culinary purposes as any other stagnant water. Many have supposed that the freezing process effectually eliminates all impurities. That such is not the case is abundantly proven by the tracing of cases and epidemics of enteric diseases to the use of impure ice water. The germs of typhoid are not destroyed by the freezing of the water in which they are present. In urban districts where ice is manufactured from distilled water it should be given the preference for all household purposes. It being remembered that boiling the water destroys the harmful microbes, and freezing does not make them uninfectious.

## DOMESTIC CORRESPONDENCE.

### LETTER FROM NEW YORK.

(FROM OUR OWN CORRESPONDENT.)

Early in July the summer Medical Corps of the Board of Health commenced their two months' service in the tenement house districts of the city. There are fifty of these physicians, one for each district, and it is their duty to make house to house visits among the poor for eight hours daily; reporting twice a week at Sanitary Headquarters, to Dr. Moreau Morris, one of the most experienced officers of the Board of Health, who has special charge of this department of its work. They are required to seek out and treat all cases of disease, especially among children, which are not already being cared for by private or dispensary physicians, to give advice in regard to the care of infants and young children in hot weather, and in regard to sanitary matters in general, and to promptly report all nuisances and all cases of contagious disease met with. They distribute in every household copies of the Board of Health's "Rules for the Care of Infants," printed in French, German and Italian, as well as English, and are also provided with free tickets for the salt water excursions of the St. John's Guild Floating Hospital, which they furnish to all suitable cases. The city appropriates $10,000 for carrying on this work, each physician receiving $200 for his two months' service, and the salaries are certainly well earned, for the duty is a most laborious one, and the mere physical exertion of climbing to the top floor of one five-story tenement house after another, in the hot weather, is no trifling matter.

There can be no question that this house to house service is of very considerable importance in keeping down to some extent the infant mortality which is, unfortunately, always so extremely large in the summer months in a great city like New York; and the special usefulness of it is no doubt to be found in the fact that by this means many cases of diarrhœal disease are discovered and treated in their incipiency, which would otherwise be permitted to run on without interference until all medical aid would be useless. The extent of the work can be readily seen from the statistics of the summer corps of physicians in the season of 1890, which are as follows: Number of tenement houses visited, 40,364; families visited, 321,012; sick cases treated; minor nuisances abated by personal effort, 5,413; complaints of nuisances forwarded, 551; circulars in regard to the care of infants distributed, 51,784; tickets to the St. John's Guild floating hospital excursions, 16,627. In the city of Brooklyn the same plan of summer service has of late years been adopted, and there are now some twenty physicians engaged in the work there.

In connection with the saving of infant mortality, it may be mentioned as a matter of interest that one of the large down-town dispensaries (the Eastern), has commenced the preparation, under the supervision of a competent physician, of sterilized milk for the use of the poor of the neighborhood. It is supplied at a low price to those who can afford to pay for it, and gratuitously to those who are unable to do so. It has been claimed by the well-known authority on the subject of milk, Dr. E. F. Brush, that children fed exclusively upon milk that has been devitalized by sterilization are not as efficiently nourished as those fed on unsterilized milk; and the remedy which he would suggest for the poor character of milk so generally in use, is reform in the dairy itself, an important feature of this reform being the spaying of all heifers to be used as milch cows. But, even if Dr. Brush is right, it will be a long time before the milk supply for the poor of our great cities is what it ought to be, and the best milk even is liable to contamination and rapid changes in hot weather. The general sterilization of milk in hot weather at least (granting that his views are correct), would therefore seem to be the lesser of two evils; and, leaving out of the question the matter of possible tubercular infection through unsterilized milk, there would undoubtedly be a decided diminution in the infant mortality from diarrhœal diseases if the various similar institutions throughout the city would follow the example of the Eastern Dispensary in supplying sterilized milk.

During the early part of July the number of deaths reported in the city, while sufficiently large, indeed, has been considerably smaller than the average for the same period during the last five years, and this is no doubt due in great measure to the unusually cool weather that has prevailed. A very large proportion of all the deaths, however, have been among children under one year, and most of these have been carried off by diarrhœal diseases. There is always, naturally, a diminution in the number of deaths from respiratory diseases at this season of the year, and there has also lately been a marked falling off in the deaths from contagious and infectious diseases. In one week recently (that ending July 4th) there were only two deaths from typhoid fever reported in the city —a most unusual and gratifying circumstance.

While the eye witnesses of the four executions at Sing

Sing prison on the 7th of July have been bound to secrecy, and no newspaper reporters were permitted to be present, it seems to be perfectly clear that on this occasion the method of electricity proved an unequivocal success, death in each instance being instantaneous and entirely painless. One of the physicians who was present at the execution of Kemmler, and also at Sing Sing, Dr. C. M. Daniels, of Buffalo, has been rather more communicative than the others, and he states that while he never had the slightest doubt that Kemmler's death was instantaneous and painless, the Sing Sing executions were a great improvement over his, being systematic, well planned, and perfectly carried out in every detail. "There was," he says, "no absence of head to direct and control, as at the Kemmler execution. Every man who took part in the execution knew exactly what was expected of him and what responsibility rested on him, and if there had been any blame to place after the executions it could not have been thrown from shoulder to shoulder, as it was after the Kemmler execution."

Dr. A. P. Southwich, the originator of execution by electricity, who was also present at Sing Sing, has expressed himself as completely satisfied with the result. He is reported as saying: "The executions were a success in every way, and there was not the slightest hitch. Electric execution has come to stay. These executions have demonstrated that the method is humane."

P. B. P.

## The Amenities of Medical Discussion.

*To the Editor:*—The readers of THE JOURNAL who are interested in the legal side of their profession may remember an article in the issue for May 2, 1891, on "Medical Cases in the Courts," in which was a paragraph entitled "Cosmetics and Spectacles in Court." They may also have noticed a savage attack on the writer in the issue for June 6, 1891, under the title of "The Optician and Ophthalmology."

It has been said that physicians show more bitterness in their discussions of unsettled questions than members of other professions, and it would seem as if this article tended strongly to prove the statement.

If reference is made to the articles it will probably be admitted by every unprejudiced person that the lawyer made a candid and even tempered statement, while the physician or ophthalmologist, if he prefers so to be styled, made a violent, and uncalled for attack and lost control of himself in the heat of the argument.

The doctor in question, Dr. George M. Gould, goes so far as to say that the article should have been excluded from the pages of a journal caring for scientific medical progress.

Perhaps the editor of THE JOURNAL will be pleased with the snub so felicitously given him, and it might be well for Dr. Gould to send a comprehensive statement of his opinions to all the medical journals of the country, and demand that nothing be admitted which varied from them.

No doubt Dr. Gould's standing in the profession is such that a request of this sort coming from so prominent a gentleman would be most humbly acquiesed in.

A considerable portion of the paragraph is quoted by Dr. Gould for the purpose of heaping contumely upon it, but he seems to be unconscious that it is merely restatement of the opinion and reasoning of the French Court on the question at issue.

Possibly he intends to ridicule the decision of this court, bnt those persons who rail at judges do not usually commend themselves to the approbation of the public.

Dr. Gould says that when a layman (referring to the writer) attempts to say anything about medicine he takes the side of ignorance and untruth and makes a mess of what he has to say.

If Dr. Gould will take the trouble to read the article he will probably find out, that the writer did not undertake to give any opinion whatever about medicine but rather about law, and possibly on such subjects he is as well qualified to speak as Dr. Gould.

Nothing was said in regard to the respective merits of the optician and the ophthalmologist in treating weak or diseased conditions of the eye, but the opinion was given that, as a matter of law, the optician, when he furnishes glasses believed to be suitable for a customer, does not violate the medical license laws of this country.

Why the expression of such an opinion, which is entirely within the proper scope of a writer on medical law, should excite such unbounded wrath it is difficult to understand, except on the theory that Dr. Gould is in some way the duly examined, licensed and appointed protector of his profession. The writer did not say or intimate in anyway that the optician knew as much about the eye as an ophthalmologist, nor did he approve even by inference of ignorance on the part of the optician. On the contrary he believes very strongly that the better medical education the optician has the more certainly will he be able to give the right glasses to a customer.

If Dr. Gould means to say that a person selling glasses in an optician's store, after endeavoring to fit them to the needs of the customer, is liable for a breach of the medical license law, he should make such a statement boldly.

The writer expressed the opposite opinion. In either case it is really an opinion about law and not about medicine, and consequently the whole of Dr. Gould's tirade finds no sound basis.

The writer, not having the fear of Dr. Gould before his eyes (in this case no glasses at all are used, the sight being perfect), reiterates his opinion that the optician who sells glasses does not violate the medical license law any more than does a person in the establishment of an artificial limb manufacturer who sells a cork leg to a customer.

If Dr. Gould believes the contrary he should at once get up a test case in his own city of Philadelphia, where there are no doubt numerous opticians who daily sell glasses to customers.

It will, however, be probably a rather disheartening drawback to him to know that he must convert the district attorney to his view before he can make much progress in the effort to root out the opticians.

HENRY A. RILEY, A.B., L.L.B., New York.

### The Hypodermatic Use of Quinine.

The following communication addressed to a subscriber so fully covers the subcutaneous use of quinine, that he has asked and obtained permission to have the same published.

*Dear Doctor:*—I take great pleasure in replying to your inquiry in regard to the hypodermatic employment of quinine. I generally use the sulphate, adding dilute sulphuric acid, drop for grain, then water enough to make one syringeful—25 to 30m. Sometimes a drop or two more of the acid is necessary. Five grains is the usual quantity for a syringeful, and in giving larger quantities (I usually give 10 gr.) I repeat the injection. The solution is always made as wanted, the injection made into muscle or deep connective tissue, as in the lumbar region. I have never seen an abscess or had one occur when given in this way. Usually there is some tenderness. In one case where the apothecary used strong $H_2SO_4$ and gave 10 gr. in one injection, in the arm, there was a small slough, hardly involving more than the epidermis. I am inclined to believe he had an excess of acid. In another case the resident divided the dose unequally and gave about 8 gr. in one side of the back, the rest, 10 gr., in the other. In the region of the first, on the following day, there was a soft, deep-seated swelling about the size of a silver dollar, with pain all over that side. The side was painted with iodine and pain and swelling subsided, though there was some left on the patient's leaving on the second day. One patient had 30 such injections with almost no pain, and without local evidence.

I have used the bi-muriate with urea, but could see no advantage. Have often intended to experiment with other solutions but so far have not done so. Quinine is given in this way very often in Texas, usually in cases where the danger of an abscess would not weigh with that of risking any other method.

I have given it often merely to see the local effect.

Very truly yours,

GEORGE DOCK.

GALVESTON, TEXAS, June 13, 1891.

### Inter-Continental American Medical Congress.

At the regular quarterly meeting of the Golden Belt Medical Society, of Kansas, held in Junction City, Kansas, July 3d, 1891, Dr. W. B. Dewees, President of the Golden Belt, having been appointed a member of the Auxiliary Committee of the Inter Continental American Medical Congress, presented the following resolutions, which were passed unanimously:

*Resolved*, That this society heartily approve of the permanent organization of the Inter-Continental American Medical Congress, believing it to be of material benefit in promoting the interests and welfare of the medical profession of America.

*Resolved*, That the members of this society will do all in their power to promote the interests and welfare of the Congress.

*Resolved*, That the Secretary of this society is hereby instructed to forward a copy of these resolutions to the *Kansas Medical Journal* and THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION for publication.

F. B. BROWN, M. D.,
Secretary Golden Belt Medical Society.

Salina, Kansas, July 9, 1891.

### Section on Materia Medica and Pharmacy.

*To the Editor:*—On behalf of the officers of the Section on Materia Medica and Pharmacy of the American Medical Association, I invite the members to prepare papers to be read at the Detroit meeting of the Association. The first meeting of this section was held at Washington last May, and proved successful in every respect. The titles of papers for the section should be submitted as soon as determined. It will not be necessary to submit the article until a date nearer the coming meeting; for further information address the secretary of the Section, DR. H. M. WHELPLEY, St. Louis.

## NECROLOGY.

### John Frederick May, M.D.

Dr. John Frederick May was born in the city of Washington, D. C., May 19, 1812, and died of pneumonia, at his residence 2022 G. street, n. w., May 1, 1891. He was the son of Dr. Frederick and his wife, Julia Matilda (Slocum) May, of this city, who were married in 1811.

Dr. Frederick May was a native of Boston, Mass., born in 1773, and a graduate of Harvard College, both in letters and medicine, writing his thesis on *Tetanus* in Latin, a copy of which may be found in the "Toner Collection," Library of Congress. He received his medical degree in 1795. He studied medicine with Dr. John Warren, brother to the patriot, Dr Joseph Warren, who was killed in the battle of Bunker Hill.

The father of the subject of this sketch settled the same year of his graduation in the, then, infant capital of the United States, five years before the archives of the Government and the executive offices were removed hither. He brought with him letters of introduction to the leading men here at that time; Daniel Greenleaf, for whom Greenleaf's Point is named; Hon. Wm. Cranch, the first presiding judge in the District of Columbia; Notly Young and others. He was a man of genteel and scholarly tastes and possessed excellent business habits, fully identifying himself, from the first, with the interests of the city. He acquired real estate and encouraged business enterprises; was one of the incorporators, and a director for building the first bridge over the Potomac river where the Long Bridge now crosses. He was, also, a promotor of the extension of the Chesapeake and Ohio canal through the city. He was an early and earnest advocate for schools, and filled the chair of Obstetrics in Columbia College from the organization of the medical department in 1825 until his resignation in 1839. For years he enjoyed the leading, and a large practice in obstetrics, and was well qualified to discharge its responsible duties. As a physician he was justly popular, and as a lecturer, entertaining, and at times, pathetically elo-

quent. His wife, Julia M., died October 14, 1823, leaving a family of eight children—six sons and two daughters. The doctor did not marry again, but devoted himself to the education of his children, and had the satisfaction of seeing them all well fixed in life before his death, which did not occur until July 23, 1847. His father—the grandfather of John Frederick—was Col. John May, a leading Boston merchant in his day, and one of the patriots of the Revolution, who, in 1773, disguising themselves as Indians, threw a cargo of tea overboard into Boston Harbor. Col. May's immediate ancestor was Capt. May, a man noted for his courage and prowess in defence of New England against the Indians. The May family came originally from England and settled in Massachusetts in 1632.

Dr. John Frederick, to whom was given the first name of both father and grandfather, came thus of brave and courageous stock, who were among the leading citizens of the community in which they lived. He received a collegiate education at Columbia College, and took his medical degree from the same institution in 1834. Shortly after this, he went to Europe, where he spent more than a year in professional study in the leading hospitals of London and Paris. In this way he familiarized himself with all the latest advances in medicine and surgery. After an extended tour through Europe, the West Indies and the United States, he began to practice in his native city. He joined the Medical Association of the District of Columbia in 1838, his father being president of that body at the time. In 1840, he became a member of the Medical Society of the District. This organization his father assisted in founding, his name appearing in the original charter of 1819, and also in the amended charter of 1838.

In 1839, Dr. John Frederick May was elected to the "Chair of Anatomy and Physiology" in the Columbian College. In 1841, he was transferred, by consent of the faculty, to the "Chair of the Principles and Practice of Surgery." This position he filled most acceptably until his resignation in 1858. He was honored, about the same time, with the professorship of surgery in the University of Maryland, which he filled for two years without vacating his college duties in Washington, and to the satisfaction of his associates in both faculties. Dr. May became also a member of the Section of Physiology and Medicine of the "National Institute" in Washington, a scientific organization antedating the existence of the Smithsonian Institution of this city. This association of physicians was active in 1843, and for a number of years sent delegates to the American Medical Association. Eventually, however, it was merged into, or its functions were absorbed by the Smithsonian Institution. The "Pathological Society of the District of Columbia" was formed about 1846, Dr. May taking an active and influential part in its discussions. In 1848, Dr. John Frederick May was united in marriage with Sarah Maria Mills, by whom he had seven children—six of whom are living, two sons and four daughters. The family of the May's have been noted, for generations, for their fine physical development, bright intellects and honorable traits of character.

In 1847, Dr. May became a member of the American Medical Association, being a delegate from the Medical Department of the Columbian College. In 1858 he was elected to the Chair of Surgery in the Shelby Medical College at Nashville, Tenn., which he filled very acceptably until the breaking out of the "Civil War."

Although Dr. May is best known by his splendid record as a surgeon, he was, throughout his professional career, a general practitioner, and was equally astute, strong and able in each and all the departments of medicine. He took, it is true, great pains to qualify himself as a surgeon, making special studies in anatomy, and equipping himself with the best instruments devised for the different operations obtainable either at home or abroad. Being a man of fine physique, over six feet in stature, and well proportioned, calm and self-possessed, with clear convictions and steady nerve, he was every way suited for the duties and responsibilities of the surgeon. His skill in surgery speedily attracted attention, and placed him in the very front rank of this field of practice in the United States. He was one of the first surgeons of America to amputate, with success, at the hip joint, and the first in Washington to perform ovariotomy. His skill in surgery was widely recognized, so that for years most of this kind of practice in the city of Washington fell to his care. He was surgeon to the Washington Infirmary, which was conducted under the auspices of the faculty of the National Medical College, in which he was the professor of surgery.

Dr. May was punctilious in his intercourse with his professional brethren and with the public, but he was at the same time the soul of honor. He was always ready, on a proper basis, to render service to the afflicted and to advise and assist the young practitioner in difficult cases. He loved his profession, and esteemed the man who, by his merits, won the approbation of the medical fraternity.

Dr. May, while he was a good surgeon and physician, was also an excellent business man, attentive to business matters, making good investment, and accumulating a large and productive estate. He was earnest and zealous in whatever engaged his attention, whether it was his profession or his business investments. In all his affairs he brought a clear, strong judgment to bear, and wasted nothing on sentiment.

Shortly after the war Dr. May removed to the city of New York. He continued, however, to spend much of his time in Washington, attending to his real estate and other business interests. His family, however, returned to reside in this city about 1880. In 1884 he was elected surgeon on the consulting staff of "Garfield Memorial Hospital." He served here faithfully and as president of the medical staff for five years, when the necessity for lessening his duties, owing to the accumulation of his years, induced him to resign.

Of late the Doctor has been a great sufferer from asthma, and particularly at certain seasons of the year. He tried various climates and many places, at the sea level and at high altitudes, but found no place affording more than a modified exemption.

One of his sons, William May, is treading in the foot-

steps of his father, having studied medicine, and being now in full practice, with a predilection for surgery.

Dr. May's funeral took place from St. Paul's Church, Twenty-third street, and was largely attended by the profession, private citizens and former friends of the Doctor's. The Rev. Dr. Harding conducted the Episcopal services. The remains of Dr. May, followed by a large cortege, were placed in Rock Creek Cemetery.

J. M. T.

## BOOK REVIEWS.

PRACTICAL PATHOLOGY AND MORBID HISTOLOGY. By HENEAGE GIBBES, M.D., Professor of Pathology in the University of Michigan, etc. Pp. 320, cloth, 8vo. Philadelphia: Lea Bros. & Co. 1891.

In the olden times, instead of writing a preface, the author wrote an "apology" for having written a book at all. One should now-a days be very sure that there is need, when he undertakes to write a new book. Our author finds the need of a new book on the subject of which he treats, in the fact that photographic illustrations of actual appearances are needed to fill an existing gap in the domain of morbid histology; and the fact must be admitted that our monographs and text-books are filled with illustrations which are rather diagrammatic, than representative of an actual specimen. There is sufficient reason why this should be the case, and that is a pecuniary one. It has been pretty well demonstrated that, in the existing condition of the art of lithography and engraving, we cannot reproduce on ordinary paper, either faithful photographs or well worked out engravings; a higher class of paper must be used; this necessarily increases the cost of the book. The recognition of this fact has hereto fore led publishers, both in Europe and America, to publish fine illustrations, when the character of the book required it, on separate sheets from those on which the text of the book was printed, and it may be safely said without successful contradiction, that the book under review affords abundant proof of the wisdom of the common practice. The publishers have endeavored to surmount the difficulty by printing the entire book on extra heavy paper, but a comparison of the illustrations will show that there is still lacking that quality which would bring out distinct tracings. Take, for instance, the illustration of spindle celled sarcoma on page 165, and it is impossible to resist the conclusion, either that the section was cut too thick, or that it was slightly out of focus when the photograph was taken. Photomicrographic negatives are notoriously difficult to reprint from, even with the most careful attention to details, owing to difficulties of illumination, and the fact that, as a rule, they are made by amateur photographers and not by experts. A comparison of the illustrations in this work with those in Peyer's Atlas of Clinical Microscopy, will show very plainly the difference between photomicrographs reproduced in the book pages and those on separately printed boards. The book itself affords an excellent example of the difference in class and clearness, if one compares the beautiful plate in the frontispiece with the out-of-focus picture on page 177.

The text of the book, so far as it relates to morbid histology, is excellent, and fairly embodies the latest teachings; that with reference to the preparation of specimens, the use of the microscope and the laboratory technique is to be commended. The chapter on practical bacteriology is one of the best in the book, which as a whole compares favorably with others on the subject of which it treats.

COLLECTED CONTRIBUTIONS ON DIGESTION AND DIET, BY SIR WILLIAM ROBERTS, M.D., F.R.S. Philadelphia: Lea Bros. & Co. 1891.

This volume consists mainly of a reprint of the author's Lumbian Lectures on the "Digestive Ferments and Artificially Digested Food," and five lectures on "Dietetics and Dyspepsia" given at the Owens College in 1885. It also contains the other publications of the author on kindred subjects. The subject matter is divided into four sections:

1. Digestion, and the Digestive Ferments.
2. Dietetics.
3. Preparation of Food for Invalids.
4. Dyspepsia.

The section on Dietetics is replete with suggestions on practical points. It discusses many points which have been observed by the clinician, but have not yet reached the more exact physiologist. These ultra-physiological facts of digestion, are of the highest importance to the practitioner, if he be familiar with the facts of digestion as already determined by the physiologist and chemist. The book is in many respects one of the most satisfactory productions on the subjects of which it treats, that has appeared in recent years.

MEDICAL REGISTER OF NEW YORK, NEW JERSEY AND CONNECTICUT, 1891-1892. Published under the supervision of the New York Medico-Historical Society, Wm. T. White, M.D., editor. Vol. 29, press of G. P. Putnam's Sons. This is certainly the most complete physicians' register that has come into our hands. It contains the names of 7,854 physicians, of which there are in New York State, exclusive of New York city and Brooklyn, 3, 65; New Jersey, 961; Connecticut, 611; New York city, 2,325; Brooklyn, 778. The increase over last year is 205. Not only does the register of physicians

seem to be complete, but there is also a large amount of useful information, tersely stated, not one line of which would we desire to have omitted.

AN ELEMENTARY HAND-BOOK ON POTABLE WATER. By Floyd Davis, M.Sc., Ph.D. Boston: Silver, Burdett & Co. 1891.

A good little book, containing chapters on Pure Water, Inorganic Constituents, Vegetable Constituents, Animal Constituents, Microörganisms, Water Supplies, Natural Purification, Artificial Purification, Systems for Central Filtration, and with an Appendix, having, under Sec. A, The Origin and Home of Cholera, and under Sec. B, Qualitative Tests for Impurities in Drinking Water.

MEDICAL PUBLICATIONS.—HARVARD MEDICAL SCHOOL. 1890.

The preface says: "The following articles are presented in bound form to show the character of the original work done by the instructors of the school, or under their personal supervision." The volume contains eighteen papers, the majority of which have had their origin in the physiological laboratory. The articles, nearly all of which have been published elsewhere, are of the highest scientific standard and are worthy of the great University which fathers them.

## MISCELLANY.

DR. W. S. CHRISTOPHER has resigned the chair of Theory and Practice of Medicine in the University of Michigan to take the chair of Diseases of Children in the Chicago Policlinic.

WANTED.—A few copies of No. 1, Vol. XV, of THE JOURNAL; will pay good price. Send to this office.

DR. H. M. WHELPLEY, professor of Microscopy in the St. Louis College of Pharmacy, and for the past five years a lecturer in the Missouri Medical College, has been elected Professor of Physiology and Histology, and Director of the Histological Laboratory of the latter institution. The doctor has also accepted the position as Secretary of the faculty.

VERMONT STATE MEDICAL SOCIETY.—The following papers will be read at the next annual meeting of the Vermont State Medical Society, to be held at Burlington, October 15 and 16:

1. Summer Diarrhœa in Children, H. D. Holton, Brattleboro. Discussion opened by C. F. Camp, Barre, G. F. B. Willard, Vergennes.

2. Predisposition to and Prophylaxis of Phthisis, A. B. Bisbee, Montpelier. Discussion opened by S. T. Brooks, St. Johnsbury, E. A. Bates, Highgate.

3. The Treatment of Phthisis, A. C. Bailey, West Randolph. Discussion opened by A. P. Grinnell, Burlington, O. W. Sherwin, Woodstock.

4. Recent Advances in Surgery, L. M. Bingham, Burlington. Discussion opened by Henry Janes, Waterbury, F. D. Robertson, St. Albans.

5. The nature, varieties, causes, symptoms and treatment of Glaucoma, J. H. Woodward, Burlington. Discussion opened by C. E. Chandler, Montpelier, F. T. Kidder, Woodstock.

6. Some European Lessons, J. H. Jackson, Barre.

7. Puerperal Septicæmia, H. R. Wilder, Swanton. Discussion opened by C. F. Branch, Newport, H. S. Brown, St. Johnsbury.

8. Heart Sounds and Cardiac Murmurs, D. C. Hawley, Burlington. Discussion opened by J. H. Jackson, Barre, D. F. Rugg, Hartland.

9. A Study of New Remedies, W. H. Vincent, Orwell. Discussion opened by E. J. Hall, Morrisville, F. R. Stoddard, Shelburne.

10. Albuminuria, it Detection and Significance, F. L. Ladue, Alburgh Springs. Discussion opened by Geo. B. Nichols, Barre, W. D. Huntington, Rochester.

11. Pure drinking water and how to secure it, J. H. Hamilton, Richford. Discussion opened by J. D. Hanrahan, Rutland, C. G. R. Jennings, Bennington.

12. Indigestion and its Relation to Disease, C. W. Peck, Brandon. Discussion opened by Edw. R. Campbell, Bellows Fall, C. W. Jacobs, Richmond.

13. Preventive Medicine, C. S. Caverly, Rutland. Discussion opened by S. S. Clark, St. Albans, E. W. Shipman, Vergennes.

14. The Upper Air Passages and their Diseases, O. B. Douglass, New York. Discussion opened by J. H. Woodward, Burlington, S. W. Goss, Chelsea.

15. Acute Capillary Bronchitis in Children, W. G. E. Flanders, Fairfield. Discussion opened by Joel Allen Johnson, E. P. Stimson, West Randolph.

16. Patent Nostrums. L. W. Flanders, Burlington. Discussion opened by H. T. J. Howe, Waitsfield, C. J. Russell, Hinesburgh.

17. Shall we Abolish the Lecture System in our Medical Schools? O. W. Sherwin, Woodstock. Discussion opened by A. T. Woodward, Brandon, Geo. Dunsmore, St. Albans.

18. President's Annual Address, J. N. Jenne, St. Albans.

This announcement is issued in order that every member of the Society may have ample opportunity to come prepared to discuss the various papers.

It is especially urged that each member appointed to discuss a paper make special preparation to do so, and that any member who will be unable to fulfil such appointment, notify the Secretary before October 1.

It is expected that other papers will be offered for the programme, and all members are reminded that volunteer papers are always in order.

The annual Banquet will take place on Thursday evening, October 15, after the President's Address, with Prof. J. H. Jackson, of Barre, as Anniversary Chairman.

There will be the usual exhibit of drugs, medicines, surgical instruments and appliances, etc.

It is expected that the usual courtesy of half-fare will be extended by the railroads.

The regular programme will be issued October 1.

D. C. HAWLEY, M.D., Sec'y.

*Official List of Changes in the Stations and Duties of Officers Serving in the Medical Department, U. S. Army, from July 4, 1891, to July 10, 1891.*

Asst. Surgeon James W. Buell is retired from active service, by reason of disability incident to the service.

Asst. Surgeon William S. Owen, Jr., detailed to attend the encampment of the Illinois National Guard, near Springfield, Ill., from July 18 to 25, and August 11 to 18, 1891.

*Official List of Changes in the Medical Corps of the U. S. Navy, for the Week Ending July 11, 1891.*

Asst. Surgeon Robert Boyd, ordered to the U. S. receiving ship "Dale." Washington, D. C.